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The Journal of

THE ROYAL AERONAUTICAL SOCIETY

JANUARY 1947 · NO 433 · VOL 51

FLY

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THE ROYAL AERONAUTICAL SOCIETY

4 HAMILTON PLACE, PICCADILLY, LONDON, W.1

JANUARY NOTICES 1947

JOURNAL PREMIUM AWARDS

The Council have set aside an annual sum of £250 for the award of premiums for papers published in the *Journal* and the Council hope that members (or non-members) will contribute papers on their own special subjects.

NEW YEAR

The President and Council extend to all members at home and overseas their best wishes for the year 1947.

CONTENTS OF THE JANUARY JOURNAL

Aeroelastic Problems at High Speed, by Professor A. R. Collar, M.A., B.Sc., F.R.Ae.S.

Power Units for Future Aircraft, by Air Commodore F. R. Banks, C.B., O.B.E., R.A.F.V.R., F.R.Ae.S.

Low Drag Aerofoils, by Dr. L. G. Whitehead, Ph.D., A.F.R.Ae.S.

Aspects of the Determination of the Strength of Materials, A Summary of Meetings of the British Rheologists' Club, by E. W. J. Mardles.

The Dangers of Nitrobenzene—Correspondence.

Review.

SPECIAL GENERAL MEETING

NOTICE IS HEREBY GIVEN that a Special General Meeting of the Voters of the Society will be held on THURSDAY, the TWENTY-THIRD day of JANUARY, 1947, at 6 p.m., in the offices of the Society for the purpose of considering and if thought fit of passing the following Resolution:—

That this meeting adopts the new Constitution proposed by the Council of the Society as the Constitution of the Society.

Note.—In accordance with a Special Resolution passed at a Special General Meeting of the Voters of the Society on the TWENTIETH day of DECEMBER, 1946:—

(*a*) Rules 104, 105, 106 and 107 have been suspended.

(*b*) Only those amendments to the proposed Statutes and Rules of which written notice has been received by the Secretary not less than seven days prior to the date of this meeting will be considered.

(*c*) Voting will be by a show of hands and a simple majority will be sufficient to carry any resolution.

By order of the Council,
J. LAURENCE PRITCHARD,
Secretary.
31*st December*, 1946.

81
714650

Light refreshments will be served after the meeting.

Main

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DEATH OF MISS HILDA LYON

It is with regret that we announce the sudden death of Miss Hilda Lyon, on 2nd December, 1946, following an operation.

Miss Lyon held a leading place in the small group of women who have made themselves a place in the aeronautical world.

Educated at Beverley High School, and Newnham College, Cambridge, where she read mathematics, she first took up stress work at Messrs. Siddeley Deasy and at Messrs. Parnall. Later in 1925 she joined Cardington and shared in the stress calculations of the R.101; the Cardington staff of that period including Prof. Baker, Dr. Roxbee Cox, Prof. Pugsley, Dr. Walker, T. S. D. Collins, and others who have since established a place for themselves in aeronautics. In 1930 she was awarded the R.101 Memorial Prize and spent the next three years on research work, first at the Massachusetts Institute of Technology in America and later under Prof. Prandtl at Göttingen University. She became an Associate Fellow of the Society in 1922.

Her career at this stage was interrupted by home claims, and from 1933 to 1937 her main occupations were domestic. In spite of this, Miss Lyon not only kept up, but enlarged, the range of her interests by assisting Prof. Duncan, then at Hull University College, in research on the flutter of wings and elastic blades.

In 1937 she joined the aerodynamic staff of the Royal Aircraft Establishment, working first in the wind tunnels on boundary layer suction and later with Mr. Gates on stability and response work, taking over from Mr. Gates the control of the theoretical stability group. Her structural experience stood her in good stead when the subject of aeroelasticity came to the fore, and her work not only laid the foundation of this very important branch of the subject, but it also provided a solution to many of the problems of aircraft stability facing designers and experimenters during the war years. She served on the Stability and Control Sub-Committee of the Aeronautical Research Council.

The outstanding feature of Miss Lyon's career lies in the continuous development of her powers. She never failed to make a valuable and important contribution to any of the many branches of aeronautics in which she worked, and her early death is a very great loss, not only to the Establishment in which she worked, but also to the Aeronautical Industry and the scientific world.

Her many friends will also feel the personal loss of a charming and exceptional woman.

THE HILDA LYON PRIZE

While at the Royal Aircraft Establishment, Miss Lyon served on the Education Committee of the Apprentices and Laboratory Assistants, and took a deep and personal interest in the welfare and education of the younger generation undergoing training at the R.A.E. Technical College. It is proposed to endow in her memory an award given annually to a student of the College, and called the "Hilda Lyon Prize." If any of her friends outside the R.A.E. would care to be associated with this, they should write to Dr. G. P. Douglas, Head of the Aerodynamics Department, Royal Aircraft Establishment.

COUNCIL MEETING

A Meeting of Council was held on 28th November, 1946, at which the following were present:—

Sir Oliver Simmonds (Vice-President), in the Chair; Mr. E. J. N. Archbold; Air Com-

modore F. R. Banks; Sir John S. Buchanan; Major G. P. Bulman; Mr. A. G. Elliott; Mr. W. S. Farren; Sir A. H. Roy Fedden (Past President); Mr. A. Gouge (Past President); Major R. H. Mayo; Mr. E. T. Jones; Captain C. F. Uwins.

Among the business transacted was the following:—The appointment of the representative on the Segrave Trophy Award Committee; the report of the Grading Committee; the report of the Technical Committee; the report of the General Purposes Committee; and the report of the Journal Committee.

THE GUGGENHEIM MEDAL

Air Commodore Frank Whittle, C.B.E., F.R.Ae.S., has been awarded the Daniel Guggenheim Medal in America. The award is for an outstanding achievement in aeronautics.

Air Commodore Whittle was awarded the Gold Medal of the Society in 1945.

SEGRAVE TROPHY

Major R. H. Mayo, F.R.Ae.S., has been appointed to represent the Society on the Segrave Trophy Award Committee. The Segrave Trophy is awarded for the best performance on land or sea or in the air during the previous year.

MEETINGS AT THE HEADQUARTERS OF THE SOCIETY

During the past month meetings have been held at the offices of The Society, 4, Hamilton Place, by the following organisations:—

Aeronautical Research Committee (on various occasions).
Aircraft Recognition Society.
British Air Charter Association.
British Standards Institution.
Guild of Air Pilots and Air Navigators.
Light Aeroplane Clubs Committee.
Meteorological Research Committee.
Observers Association.
Society of British Aircraft Constructors.

ASSOCIATE FELLOWSHIP EXAMINATIONS

Members are reminded that the new syllabus for the Associate Fellowship Examinations will come into force on the 1st January, 1948.

ANNUAL SUBSCRIPTIONS OF MEMBERS

Members are reminded that their subscriptions became due on January 1st, 1947.

The present subscriptions are:—

	Home. £ s. d.	Abroad. £ s. d.		Home. £ s. d.	Abroad. £ s. d.
Founder Members ..	2 2 0	2 2 0	Graduates (age 21-25)	2 2 0	2 2 0
Fellows	4 4 0	3 3 0	Graduates (age 26-28)	2 12 6	2 12 6
Associate Fellows ..	3 3 0	2 2 0	Students	1 1 0	1 1 0
Associates	2 2 0*	2 2 0*	Companions ..	2 2 0	2 2 0

* £1 1s. 0d. without Journal.

JOURNAL BINDING

Owing to increased costs, the prices of binding of Journals will be as follows:—

1946 Volume 12/6.
Previous Volumes 13/6.

Journals should be sent direct to The Lewes Press, Friars Walk, Lewes, Sussex, and the remittance to the Secretary at the office of the Society.

CHANGES OF ADDRESS

To assist in keeping the records of members correct and up to date the Secretary will be glad if all members will notify him as soon as possible of changes of address.

When notifying changes please give the following particulars:—

Name (in block letters).
Grade of membership.
New address (in block letters).
Old address.

LECTURE PROGRAMME

The following are the first two lectures of the Spring Session, 1947. Members are admitted without tickets, but non-members must obtain tickets through a member.

The lectures will be held in the Lecture Hall of the Institution of Civil Engineers, Great George Street, S.W.1 (by permission of the Council of the Institution). Tea will be served at 5.30 except where otherwise stated.

Thursday, 16th January, 1947—At 6.30 p.m., a Film Show with Commentary by Mr. William Courtenay, M.M., A.R.Ae.S., "In Occupied Japan." Tea will be served at 6 p.m.

Thursday, 30th January, 1947—At 6 p.m., "War-time Developments in Air Transport," by Air Marshal the Hon. Sir Ralph Cochrane, K.B.E., C.B.

GRADUATES' AND STUDENTS' SECTION

Friday, 10th January, 1947—Dance at the Paviours Arms, Page Street, Westminster. Tickets, price 4/6 single, 8/- double, may be obtained from the Hon. Secretary, J. G. Roxburgh, 62 Fitzjohn's Avenue, London, N.W.3, or from the local Section Representative.

Friday, 24th January, 1947—The Aerodynamics of High Speed Flight, by A. D. Young, Esq., Senior Lecturer in Aerodynamics at the College of Aeronautics.

Tuesday, 25th February, 1947—The Role of the Experimental Department, by G. R. Edwards, Esq., Chief Designer of Vickers-Armstrongs, Ltd.

Friday, 14th March, 1947—Developments in Propeller Turbine Engines, by G. J. C. Davies, Esq., of the Bristol Aeroplane Company.

All meetings will be held at 7.30 p.m. in the Library of the Society, 4, Hamilton Place, W.1.

BRANCH LECTURES AND NOTICES

Belfast Branch

7th January, 1947—Gliding, by Professor G. T. R. Hill, F.R.Ae.S.

24th January, 1947—Smoking concert.

11th February, 1947—Electrics in Aircraft, by Mr. Woodford, A.F.R.Ae.S.

11th March, 1947—Gas Turbines and Jets, by Mr. J. Hodge, A.F.R.Ae.S.

All lectures will be held in the Central Hall, Belfast College of Technology.

Birmingham Branch

23rd January, 1947—History and Development of the Spitfire from a pilot's point of view, by J. K. Quill. At the Birmingham Chamber of Commerce at 7 p.m.

19th February, 1947—Debate with Coventry Branch. At the Birmingham Chamber of Commerce at 7 p.m.

27th March, 1947—An historical survey of the aeroplane, by Lord Brabazon of Tara. At Queen's College, Paradise Street, Birmingham, at 7 p.m.

Bristol Branch

7th January, 1947—Brains Trust, with Captain J. Laurence Pritchard, Hon.F.R.Ae.S., as Question Master.

28th January, 1947—Large Aircraft, by A. E. Russell, Esq., B.Sc., F.R.Ae.S., Chief Engineer, Bristol Aeroplane Co. Ltd., Aircraft Division.

18th February, 1947—Radar—Its Uses and Possibilities in Civil Aviation, by F. R. Willis, Esq., B.Sc., A.M.I.E.E., A.F.R.Ae.S., Radio Development Section, B.O.A.C.

4th March, 1947—Some Aspects of Stability and Control, by Prof. A. R. Collar, M.A., B.Sc., F.R.Ae.S., Professor of Aeronautical Engineering, University of Bristol.

25th March, 1947—Competition for the best paper read by a Junior Member.

15th April, 1947—Annual general meeting and film show.

All meetings will be held in the Conference Room, Filton House, Bristol Aeroplane Co., Ltd., Filton, at 5.30 p.m.

Derby Branch

7th January, 1947—Conversazione, at which Group Captain Donaldson will be present. At 5.30 p.m. in the Welfare Hall, Rolls-Royce, Ltd.

Gloucester and Cheltenham Branch

10th January, 1947—Liquid Springing, by Mr. R. H. Bound, F.R.Ae.S. At Dowty Equipment, Ltd., Arle Court, Cheltenham, at 7.30 p.m.

Portsmouth Branch

24th January, 1947—General Discussion on the Future Prospects of Civil Air Transport, by Sir Frederick Handley-Page, C.B.E., F.R.Ae.S., Sir Harold Hartley, C.B.E., F.R.S. (British European Airways), Air Vice-Marshal D. C. T. Bennett, C.B.E., F.R.Ae.S. (British South American Airways), W. Tye, Esq., A.F.R.Ae.S. (Air Registration Board).

The meeting will be held at 7 p.m. at Foresters Hall, Fratton Road, Portsmouth. Speakers will be introduced by The Lord Mayor of Portsmouth, Councillor J. Winnicott, J.P.

SOUTHAMPTON BRANCH

8th January, 1947—Elementary Metallurgy of Aircraft Materials, by A. Black, Assoc.Met., M.I.M.

29th January, 1947—Installation of Radio Equipment in Aircraft, by W. T. Davies, A.M.I.E.E.

19th February, 1947—Problems in Fighter Design, by R. L. Lickley, B.Sc., D.I.C., F.R.Ae.S.

19th March, 1947—High Speed Flight, by R. Smelt, M.A., A.F.R.Ae.S.

9th April, 1947—An Address by E. F. Relf, C.B.E., F.R.S., F.R.Ae.S.

All lectures will be held at 7 p.m. in the Physics Lecture Theatre, University College, Southampton.

ELECTION OF MEMBERS

The following members have been elected recently:—

Associate Fellows

Norman Alexander Adler (from Associate), John Taber Appleby (from Graduate), Reginald Alfred Charles Brie, Alan Breedon Malcolm Brush (from Graduate), Thomas Cochrane Campbell, James Millar Corry, Douglas George Coupe (from Associate), Francis William Free (from Graduate), Guy Clavell Inge Gardiner (ex-Graduate), Sydney James Garvey (from Graduate), Roy Wilson Gray, Arthur Howarth Green, Percy George Eynon Hand, Frank Paul Higton, Clifford George James, Stanislaw Januszewski, Raymond Albert Henry Johnson, Percival Stephen Lewis, Donald McLean (from Graduate), Edgar Vernon Marchant, Robert Bulkeley Marsh (from Graduate), Ernest Edward Marshall, Kenneth Mitchell, Albert Edmond Moreillon, Leslie Morgan (from Student), Allan Floyde Newell, Robert Maxwell Seddon, William Morey Simms (from Associate), Kenneth John Spooner (from Graduate), William Cecil Steel, William John Strang (from Graduate), Henry William Trevaskis, Geoffrey Light Wilde (from Graduate).

Associates

Charles Maurice Adams, John Howard Bassett, Leslie Kenneth Britton, Charles Henry Brown, Charles Henry Budge, Robert Ellis Chapman (from Graduate), William George Coxon (from Student), Jan De Wit, Alexander Ewing Crawford Denovan, George Edmund John Dickinson, Bernard James Dobbin, Robert Welsford Durrant; Edward Eric Elkerton, Norman Elliot, John Edwards Charles Eve, Ian George Oldland Fenton, Cyril Samuel Charles Ferriman, Arthur Henry Fisher, Douglas Howard Garside, Arnold Hawkins, Reginald John Hawkins, Leonard John Jenkins, Henry George Jones, Percy Reginald Levy (from Graduate), Geoffrey Lindley, Edward John McBirney, Desmond Walter Mitchell, (from Student), Thomas Herbert Morgan, Ian Howard Grant Nicholson, Harihar Pandurang Pathak, Gordon Stafford Rees, John Reid, Owen Sydney Staveley Roberts (from Student), Richard Gabriel Robinson (from Student), Alfred Wiliam John Smith (from Student), Leo Sydney William Smyth, Cecil George Patrick Trevelyan, Douglas Lyell Wallis, Kenneth William Watson.

Graduates

Kenneth Leslie Buckle, Hamed Kamal Eldin, Arthur Francis Houghton, Wellington Weichun Hu, Arthur James, Gordon Jefferson (from Student), Harold Barry Lee (from Student), Sidney Lipman (from Student), Stanley Ian Robertson Nicol, Douglas Edward Pay, Lewis Raymond Phillips (from Student), Richard David Richardson, Reginald Norgate

Robilliard (from Student), Hedley Derwent Scorer (from Student), Theo Vincent Small, Geoffrey Charles Staughton, Donald Bull Swinbanks (from Student), Gregory Frank Webb (from Student), Sidney William Henry Wood.

Students

James Arnold Forrest Allan, David Charles Armstrong, Gerald Sidney Ashfield, Arthur Leonard Baker, John Charles Beadle, Paul John Bellerby, Emmanuel Berg, Peter Ward Brown, Charles Basil Budd, Jacob Alexander Cartmel, Vernon Wilfred Clarkson, Joan Lidster Davis, Edward Derek Dixon, Jean Rosemary Duncan, John Alexander Elric, Eric James Few, Terence John Floyd, Geoffrey Bruce Foxall, Arthur Hesling Gibson, Laurence Jesse William Hall, Bernard Stephen Hansom, Moshe Hazan, Maurice Karl C. Henriques, Douglas Frederick Holwill, Ronald Hooper, Denis Howe, Edwin Karlsen, Gavin Stuart Kermack, Gilroy Reginald Ketley, Arthur Kingsnorth, Vernon Sydney John Lockie, Alan James Mann, Ignacy Meitlis, Thomas Charles Weguelim Micklem, John Arthur Miles, Derrick George Murkett, Denis John O'Connor, Gordon Albert Pullinger, Edward Albert Ridge, Alan William Robertson, Moira Anne Robson, John Peter Rowles, Francis Arthur Smith, John Hugh Smits, Roy Fred Robert Storey, John Alan Swingler, Cyril Richard Taylor, Arthur Montgomerie Thompson, James Gillette Try, Anthony Charles Wemyss, Michael Lloyd Wheeler, Dorothy Winifred Whitfield, James Lanham Wildhaber, Brian Wilkinson-Cox, Leslie Frederick Wilton, Kathleen Wimpenny, Robert Anthony Woodhouse Wild, Clifford Yeowart.

Companions

Richard Du Cane, Ian Stewart Lloyd, Rajnarayan Mitra, C. Rupert Moore, Samuel Henry Seed.

ACKNOWLEDGMENTS

The Council acknowledge with grateful thanks the gift of a photograph of the Tudor II from Mr. E. J. N. Archbold, Graduate, and back numbers of the Journal from Mr. D. D. Jones, Student.

ADDITIONS TO THE LIBRARY

Pamphlets in italics with location reference following in brackets. Books marked * or ** may not be taken out on loan.

A.a.328.—Experiments in Aerodynamics. S. P. Langley. Smithsonian Institute. 1946.

B.e.43.—*Jet propulsion motors for flying aeroplane models, helicopters and motor boats. Aircraft Jet and Rocket Corporation.* 1946.

B.f.95.—Vols d'Oiseaux. J. F. Ormond. F. Roth Lausanne. 1946.

D.a.94; Y6.—*Agreement concerning air communications to, through and from Great Britain and Norway. H.M.S.O.* 1946.

E.a.17.—Engineering and aircraft limits, fits and tolerances. H. Parkinson. Pitman. 1942.

EE.b.84.—Aircraft engines of the world. Paul Wilkinson. Pitman. 1946.

EE.h.26.—*Theory and testing of jet propulsion motors and rockets. S. Fonberg. Astral Aero Model Co.* 1945.

I.b.19.—Analytic geometry and calculus. Second edition. H. B. Phillips. Wiley. 1946.

L.d.115.—*Notices to Airmen.* Nos. 185, 186, 187, 188, 189, 190, 191, 192, 193, 194, 195, 196, 197, 198, 199, 200, 201, 202, 203, 204, 205, 206, 207, 208.

L.k.61.—Human factors in air transport design. R. A. McFarland. McGraw Hill. 1946.
M.c.74.—Radio Navigation. W. J. D. Allan. Chemical Publishing Co. 1941.
S.b.152.—We speak from the air. Broadcasts by the R.A.F. H.M.S.O. 1946.
S.c.17.—The German Air Force. Wing Commander Asher Lee. Duckworth. 1946.
S.e.125.—Low Attack. Wing Commander J. de L. Woolridge. Sampson Low. 1946.
S.e.127.—British Navies in the Second World War. Admiral Sir W. M. James. Longmans. 1946.

UB.5.—*B.I.O.S. Final Report No.* 525. *Poppet valves for automotive and aircraft engines. L. E. Bogue and Norman Hoertz.*

UB.5.—*B.I.O.S. Final Report No.* 42. *German oil storage blending and filling installations. C. A. Harrison and E. Laidlaw.*

UB.5.—*F.I.A.T. Final Report No.* 512. *Survey of low voltage, air circuit breaker practice. Germany.*

UB.5.—*F.I.A.T. Final Report No.* 600. *Air filters and oil filters for engines.*

UB.5.—*F.I.A.T. Final Report No.* 609. *High power radar Jagdhaus.*

A.R.C. Reports and Memoranda

2013.—*The experimental determination of the boundary layer and wake characteristics of a Piercy* 12/40 *aerofoil, with particular reference to the trailing edge region. J. H. Preston. N. E. Sweeting and D. K. Cox.*

2054.—*Vibration of propeller due to aerodynamic forces. Parts I and II. F. Postlethwaite, B. C. Canter, W. G. A. Perring, J. R. Foreshaw, H. B. Squire and F. J. Bigg.*

2068.—*High-speed flow in smooth cylindrical pipes of circular sections. A. D. Young and N. E. Winterbottom.*

2069.—*A shortened method of airscrew strip theory calculation for high-speed aeroplanes. R. C. Parkhurst and E. M. Love.*

N.A.C.A. Technical Memoranda

1067.—*Minimum energy loss propeller. N. Poliakov.*

1078.—*Experimental and theoretical investigations of cavitation in water. J. Ackeret.*

1079.—*Some basic laws of isotropic turbulent flow. L. G. Loitsianskii.*

1081.—*On the automatic regulation of output in centrifugal compressors. V. F. Ris.*

1085.—*Calculation of turbulent expansion processes. Walter Tollmein.*

Publications Scientifiques et Techniques du Ministère de l'Air

108.—*Bulletins des Services Techniques. Theorie des compas magnetiques et des indicateurs de cap utilisées en aviation.*

N.A.C.A. Technical Notes

1001.—*Lightning discharges to aircraft and associated meteorological conditions. L. P. Harrison.*

1005.—*Correlation between strength properties in standard test specimens and molded phenolic parts. P. S. Turner and R. H. Thomason.*

1006.—*Velocity distribution on wing sections of arbitrary shape in compressible potential flow. Symmetric flows obeying the simplified density-speed relation. Lipman Bers.*

1042.—*Performance test of wire strain gages IV—axial and transverse sensitivities. William R. Campbell.*

1047.—*Clamped long rectangular plate under combined axial load and normal pressure. Ruth M. Wooley, Josephine N. Corrick and S. Levy.*

1049.—*Analysis of available data on the effects of tabs on control-surface hinge moments. S. M. Crandall and H. E. Murray.*

1051.—*Preliminary investigation of the loads carried by individual bolts in bolted joints. M. B. Tate and S. J. Rosenfeld.*

1054.—*Impact strength and flexural properties of laminated plastics at high and low temperatures. J. J. Lamb, I. Albrecht and B. Axilrod.*

1060.—*An investigation of additional requirements for satisfactory elevator control characteristics. W. H. Phillips.*

1061.—*Wind-tunnel investigation of effect of wing location, power, and flap deflection on effective dihedral of a typical single-engine fighter-airplane model with tail removed. Warren A. Tucker.*

1062.—*Tank tests to determine the effect of varying design parameters of planing tail hulls I—effect of varying length, width, and plan-form taper of afterbody. J. R Dawson, R. C. Walter, and Elizabeth S. Hay.*

1064.—*Lifting-surface-theory results for thin elliptic wings of aspect ratio 3 with chordwise loadings corresponding to 0.5 chord plain flap and to parabolic-arc camber. S. M. Crandall.*

1073.—*Note on the theorems of Bjerknes and Crocco. T. Theodorsen.*

1074.—*Tests of a full-scale horizontal tail surface in the Langley 16-foot high-speed tunnel. C. F. Schueller, P. F. Korycinski and H. K. Strass.*

1034.—*Bending-torsion flutter calculations modified by subsonic compressibility corrections. I. E. Garrick.*

1052.—*Wind-tunnel investigation of effect of canopies of directional stability characteristics of a single-engine airplane model. R. MacLachlan and J. Levitt.*

1053.—*Use of ducted head baffles to reduce rear row cylinder temperatures of an air-cooled aircraft engine. M. A. Sipko, C. B. Cotton and J. B. Lusk.*

1055.—*Comparison of two-dimensional air flows about an N.A.C.A. 0012 airfoil of 1-in. chord at zero lift in open and closed 3-in. jets and corrections for jet-boundary interference. R. H. Wright and C. P. Donaldson.*

1056.—*The effects of various parameters on the load at which spray enters the propellers of a flying boat. J. R. Dawson and R. C. Walter.*

1065.—*Consideration of dynamic loads on the vertical tail by the theory of flat yawing manœuvres. J. Bosher and Philip Davis.*

1066.—*Stresses around large cut-outs in torsion boxes. Paul Kuhn and E. M. Moggio.*

1067.—*Comparison of several methods of predicting the pressure loss at altitude across a baffled aircraft-engine cylinder. J. Neustein and L. J. Schafer.*

1132.—*An electron-microscope study of used nitrided-steel piston rings. T. P. Clark and W. A. Vierthaler.*

1068.—*Effects of axial-plane curvature and passage area variation on flow capacity of radial-discharge impeller with conventional inlet buckets. W. K. Ritter, A. Ginsburg and A. G. Redlitz.*

1069.—*Effect of the N.A.C.A. injection impeller on the mixture distribution of a double-row radial aircraft engine. F. E. Marble, W. K. Ritter and M. A. Miller.*

1071.—*Wind-tunnel investigation of boundary-layer control by suction on the N.A.C.A. 653-418, $a = 1.0$ airfoil section with a 0.29-airfoil-chord double slotted flap. John H. Quinn.*

1072.—*Effect of brake forming on the strength of 24S-T aluminium-alloy sheet. G. J. Heimerl and Walter Woods.*

1075.—*Effect of compressibility on section characteristics of an airfoil with a round-nose slotted prise aileron. A. A. Luoma.*

1084.—*The effect of simulated icing on propeller performance. B. T. Corson and J. D. Maynard.*

1089.—*High-altitude flight cooling investigation of a radial-air-cooled engine. E. J. Manganiello, M. F. Valreino and E. Barton Bell.*

1124.—*Effect of small deviations from flatness on effective width and buckling of plates in compression. Pai C. Hu, E. E. Lundquist and S. B. Batdorf.*

1125.—*Effect of ratio of rivet pitch to rivet diameter on the fatigue strength of riveted joints of 24S-T aluminium-alloy sheet. H. Crate, D. W. Ochiltree and W. T. Graves.*

1127.—*A discussion of the application of the Prandtl-Glauert method to subsonic compressible flow over a slender body of revolution. Lester Lees.*

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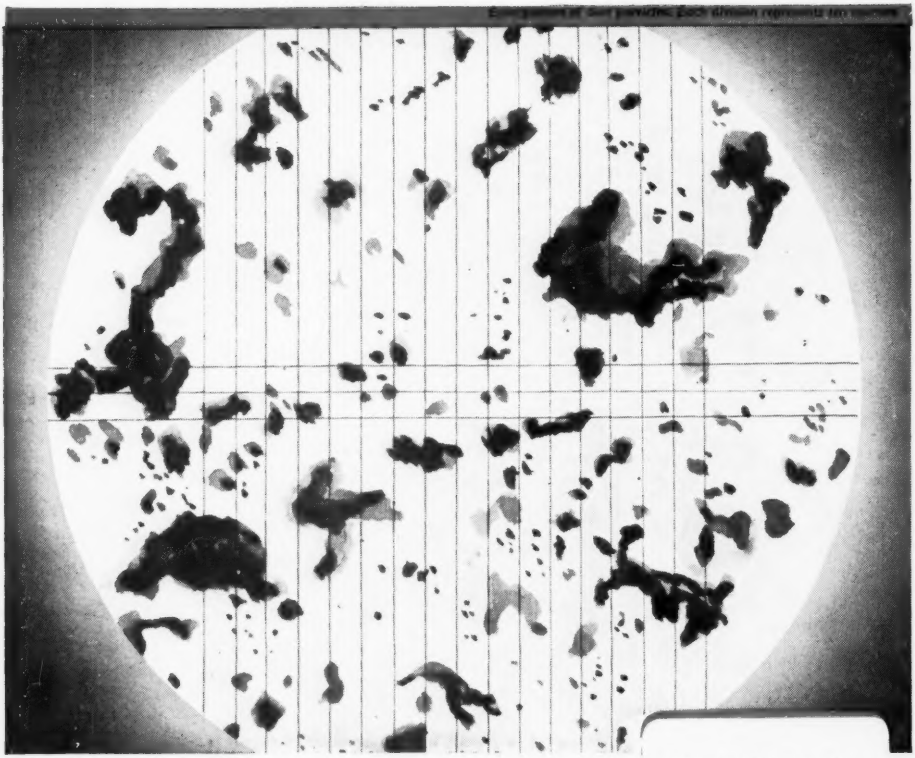

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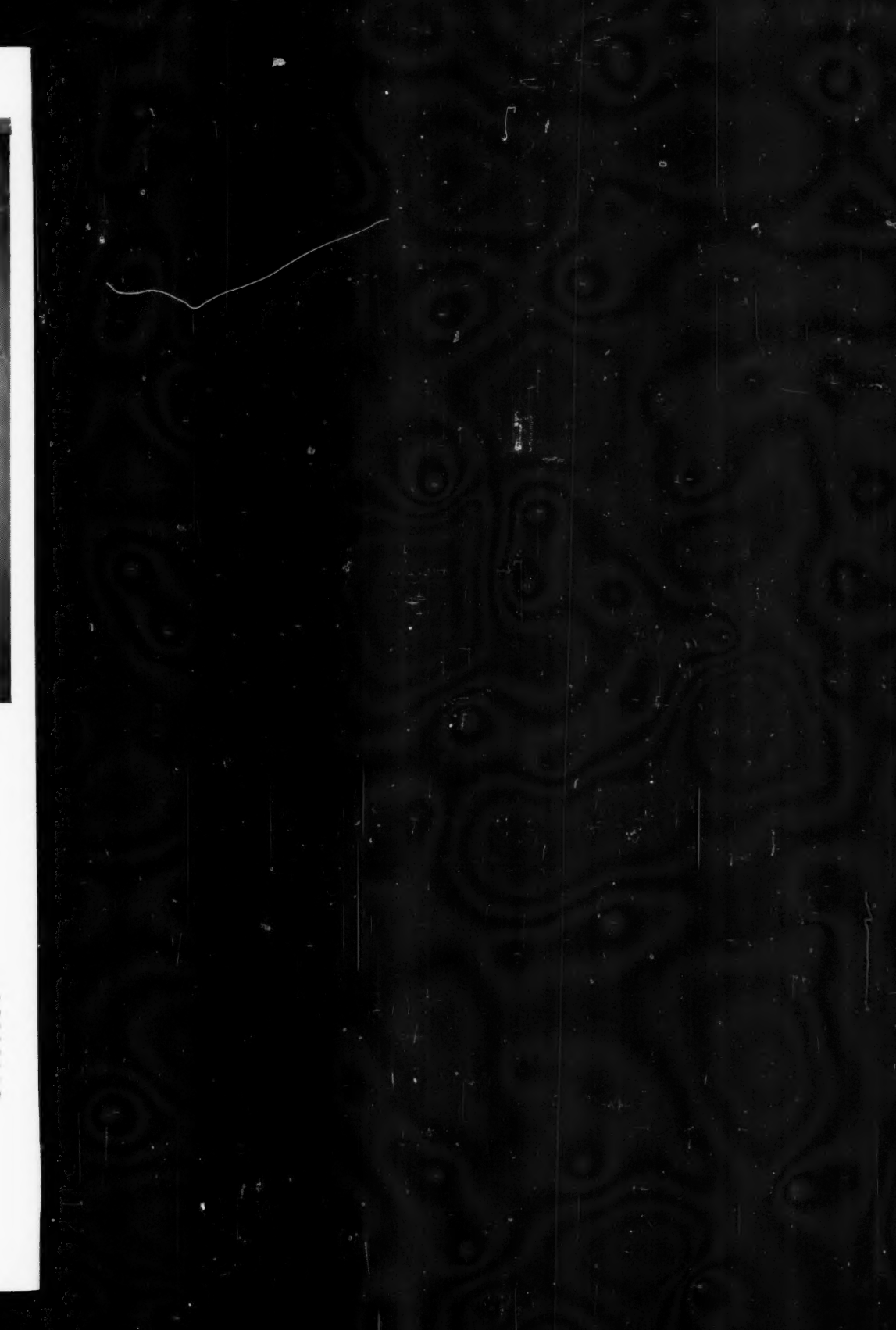

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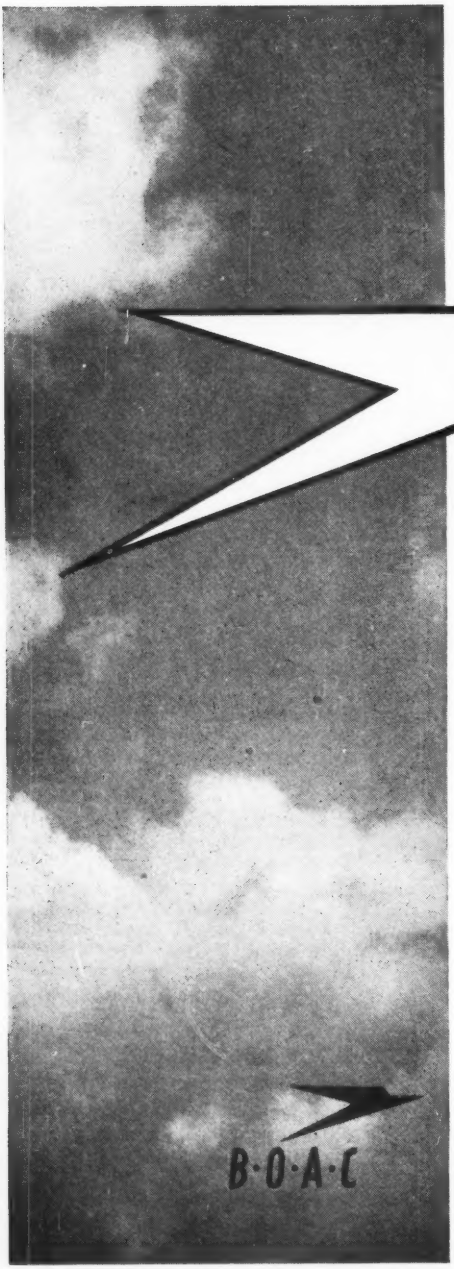

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AEROELASTIC PROBLEMS AT HIGH SPEED

by

A. R. COLLAR, M.A., D.Sc., F.R.Ae.S.

Professor Collar took his degree at Cambridge with honours in mathematics and physics; he also took an honours degree in physics at London University. In 1929 *he joined the Aerodynamics Department of the National Physical Laboratory. At the beginning of the War he was specially attached to the Staff of the Royal Aircraft Establishment to take charge of a special group investigating aeroplane flutter and vibration problems. He was appointed Sir George White Professor of Aeronautical Engineering in the University of Bristol, his present position, in* 1945.

A MEETING of the Society was held in the Great Hall of the Institution of Civil Engineers, Great George Street, Westminster, S.W.1, on Thursday, 31st October, 1946, at which a paper on "Aeroelastic Problems at High Speed," by Professor A. R. Collar, M.A., D.Sc., F.R.Ae.S., was presented and discussed. In the Chair, Dr. H. Roxbee Cox (Vice-President).

The Chairman first conveyed the apologies of the President, Sir Frederick Handley-Page, who was unavoidably prevented from attending.

The Chairman was particularly happy to preside because long ago he had ventured to work in the field of aeroelasticity, in which Professor Collar had since worked with such distinction.

On leaving Cambridge in 1929, Professor Collar went to the National Physical Laboratory, where he was almost immediately associated with the two pioneers in wing flutter theory, Dr. Frazer and Dr. Duncan. He had become involved in very deep mysteries indeed, and had even gone to the length of publishing, with those two gentlemen, the book "Elementary Matrices."

Thereafter, during 1941, he had left the Teddington cloisters for the "impurer air" of the Royal Aircraft Establishment, and there he had gained further laurels in his successful career in the difficult domain of aeroelasticity. Recently he had been appointed to the Sir George White Chair of Aeronautical Engineering at Bristol University.

1. INTRODUCTION

My lecture to-night is concerned with aeroelastic problems at high speed. This is a very large subject, and in the compass of a single lecture it is not possible to do more than touch the fringes of some of the problems. Not so long ago it might have been possible to attempt to describe briefly the full impact of aeroelastic considerations on aircraft design. For aeroelasticity was then limited, in the main, to flutter and divergence and to loss and reversal of aileron control, while speeds were not so high that compressibility effects were signifi-

cant. But nowadays we can add to those mentioned above, as urgent problems, the many ways in which deformation may affect aeroplane stability and control (particularly longitudinal stability), the effects of gusts and landing impacts, the effects of panel deformation on control forces, and other phenomena; in fact, aeroelasticity is becoming identified with the study of the stability and control, in all its phases, of a deformable aircraft.

Not only has the list of phenomena demanding consideration lengthened in this way, however. The subject has widened also, since attention must be given to differing speed ranges. To the "incompressible" speed range we must add three other ranges: the high subsonic, the sonic, and the supersonic ranges. Finally the subject has broadened as well, since we no longer restrict ourselves to conventional aircraft; unconventional aircraft (particularly those with swept-back wings), guided missiles, and the like, must also claim a share of our attention.

In a recent communication to the Journal of the Society[1] I tried, by means of a review of the progress of work in the aeroelastic field in the past ten years, to show how wide is the present scope of aeroelasticity, and how it is in fact tending to merge with the subject of stability and control of a deformable aircraft. From the practical point of view, this expansion is reflected in the rapidly growing number of stiffness and mass-balancing requirements which the aircraft designer has to satisfy, as part of the price to be paid for the high speeds which have resulted from the intensive development of the war years. We recognise clearly that the achievement of these high speeds is only the arrival at a milestone on the road of speed: almost without pausing for breath we are setting out on the next stretch, the excursion into sonic and supersonic speed ranges, with a very real hope that we may achieve those speeds in the not-so-distant future.

Throughout this continuing and rapid advance we find aeroelasticity playing a more and more important part. To-night, therefore, I am going to try to pick out, from the wide range of problems confronting us, just a few of the more important subjects: those which have considerable current interest and which, between them, exhibit most of the important features of present-day aeroelasticity. It should be recognised that the treatment is illustrative only, and that, particularly in the case of effects on stability and control and of flutter, no more than a broad outline of part of the problems is attempted.

2. LOSS AND REVERSAL OF AILERON CONTROL FOR A CONVENTIONAL AIRCRAFT

Let us first consider the effect of wing twisting on aileron control. I have chosen this as one of my illustrations of aeroelastic problems at high speed, not only because it remains a matter of great importance, but because (as we shall see later) it illustrates in striking fashion what may be the consequences of transition from the "incompressible" speed range through the subsonic and the sonic into the supersonic speed range.

Some fifteen years ago, when Roxbee Cox and Pugsley first recognised aeroelasticity as a co-ordinated science, reversal of aileron control was a major part of the subject.[2/3] In the early years, however, the stiffness provisions required to ensure that the aileron reversal speed of an aircraft should exceed the maximum diving speed by an adequate margin were not so severe as to represent a major design consideration. In consequence it was expedient to adopt a simplified method of calculation of the reversal speed; and for this purpose the method devised by Hirst [4] proved simple and adequate, and held the field for many years.

One feature of the method was the borrowing from flutter analysis of the concept of semi-rigidity, according to which the elastic body, with its infinite number of degrees of freedom, is replaced by a body having only a finite number of freedoms. In Hirst's

method only one freedom, an arbitrary torsion mode, is allowed. This has very great advantages for purposes of analysis, of course; and the error introduced by the assumption is usually not great provided that the stiffness employed in the calculation is that appropriate to the semi-rigid mode. But in fact the stiffness used is that found in a conventional stiffness test, which is always less than that appropriate to the semi-rigid mode or to the true elastic mode of distortion under the distributed aerodynamic load.[5] In the past this difference has been ignored: it was treated as a useful "hidden margin" of stiffness since it erred on the side of safety, while the error might be as small as three or four per cent and was not usually greater than ten per cent.

In the last year or so this fortunate position has changed appreciably. Diving speeds have risen rapidly; there has been an increasing insistence on high rates of roll; compressibility effects have had to be anticipated; and pronounced wing taper has become more common. All these changes have made the stiffness requirements more and more difficult to meet, so that it became anomalous to countenance the existence of a "hidden margin" of which the breadth was uncertain but which, nevertheless, was known to be widening. It was the possibility of excursion into the supersonic field which finally urged the need for new methods in which all possible account should be taken of the relevant factors; for the provision of a very high degree of torsional stiffness on a wing with a thickness only a few per cent of the chord is obviously a major design consideration.

The method which has been devised for this purpose is described in R.A.E. Report S.M.E. 3322,[6] and a few remarks will suffice here. It is an iterative method for the solution of the integral equation giving the unknown distortion mode; and the distribution of torsional stiffness from root to tip is fully taken into account. The solution may be obtained, moreover, not only for the reversal condition but for any assigned ratio of the actual rolling velocity to the rolling velocity which would be obtained from an infinitely stiff wing. It is therefore possible to derive a curve of rolling power against forward speed: this is of particular importance in relation to supersonic speeds, since there may be more than one reversal speed.

It is necessary to remark, however, on the limitation of our knowledge of the aerodynamic forces which play a part. First, the method is essentially a strip theory process, finite chordwise strips being used for convenience in place of the infinitesimal strips appearing in the integrations: and the loads on these chordwise strips are derived from two-dimensional aerodynamics. This procedure is very largely justifiable, since reversal phenomena depend principally on the aerodynamic coefficient m and on the ratio of the coefficients a_1 and a_2,* and both a_2/a_1 and m are independent of aspect ratio.[7] It follows that, provided the two-dimensional values of a_1, a_2 and m are known, the method may be expected to work well; and such evidence as exists supports this, as we shall see later.

When we come to introduce the effects of compressibility, however, the picture is much less rosy: and in fact we have to be distinctly arbitrary. In the subsonic range, there exists Glauert's theoretical analysis[8] and a small amount of experimental evidence on the effects of compressibility on a_1, a_2 and m; in the supersonic range, Ackeret's theory has been extended[9] to deduce the variation with M of a_1, a_2 and m (and also of b_1 and b_2) but experimental confirmation is almost non-existent, at least for a_2 and m; and in the sonic range there is neither theory nor experiment to help us.

Accordingly, in an application of the new method to the calculation of the rolling power of a typical elliptic wing, described in R.A.E. Report S.M.E. 3347,[10] it is assumed that we may apply Glauert's cor-

* $m = -\dfrac{\partial C_m}{\partial \xi}$ (C_L const.); $a_1 = \dfrac{\partial C_L}{\partial \alpha}$; $a_2 = \dfrac{\partial C_L}{\partial \xi}$

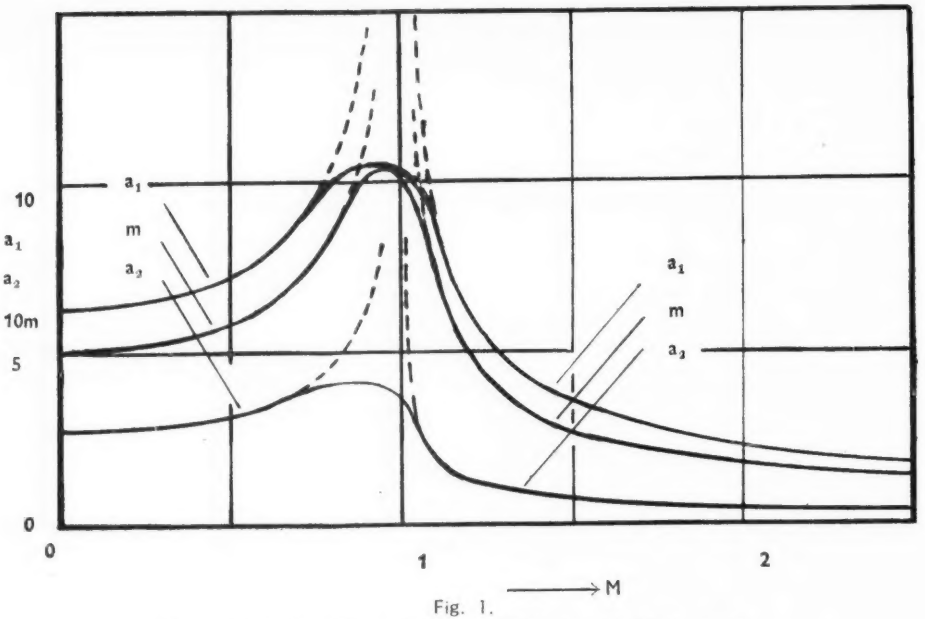

Fig. 1.
Assumed variation of aerodynamic coefficients with Mach number.

rection in the subsonic range, and that in the supersonic range also we may use theoretical air forces. In the intermediate (sonic) range we are frankly arbitrary, our only guide being the knowledge that whereas both subsonic and supersonic theories give forces tending to infinity as M approaches unity, the forces must in practice be finite; and we join the two curves to give a line reminiscent of the curve of drag coefficient of a projectile, which has been carefully measured in experiments on external ballistics.

We may offer a slight further justification of our theoretical compressibility corrections. While it is true that neither a_1, a_2 nor m individually follow the Glauert law—there are in fact very wide variations—yet it has been demonstrated[11] that the product a_2/a_1m, which enters strongly into reversal calculations, behaves approximately as if the individual coefficients were governed by the Glauert correction. Similarly, it may be argued that this product will probably conform fairly well with theory in the supersonic range.

Figure 1 shows the assumed variations with M of the coefficients a_1, a_2 and m. On "incompressible" aerodynamics each would be a constant as the speed increased (scale effect being ignored); in the curves shown the constants are divided by $(1-M^2)^{\frac{1}{2}}$ to allow for compressibility in the subsonic range, up to about $M=0{\cdot}75$. Beyond this, each is arbitrarily joined to its supersonic extension, which is a constant divided by $(M^2-1)^{\frac{1}{2}}$. It is to be noted that the constants in the supersonic range are different from those appropriate to low speeds.

The resulting rolling power for the given elliptical wing of Ref. 10, as a function of Mach number, is shown in Fig. 2. It is expressed in the non-dimensional form $ps/\xi a$, so that for a wing of semi-span s, a given aileron angle ξ and a given value of the speed of sound a, the ordinate is proportional to the actual rolling velocity p. We may first remark the two straight lines

in Fig. 2. They represent the rolling power of a rigid wing. This depends only on the ratio of a_2 to a_1; and in accordance with our assumption that each factor is modified by compressibility in the same way, we deduce that the rolling power of a rigid wing is unaffected by the compressibility correction and thus is proportional to forward speed, *i.e.* to M.* But the value of a_2/a_1 in the supersonic range is very much less than in the subsonic range,[9] and accordingly the rolling power, as expressed by the slope of the quantity $ps/\xi a$, is much less in the supersonic than in the subsonic range even for a rigid wing. From $M=0{\cdot}75$ to $M=1{\cdot}15$ the two straight lines are connected by a curve which is the result of the arbitrary variations given to a_1 and a_2 in the same range.

We now add the effect of wing twist; and the resulting rolling power is given by the second continuous curve in Fig. 2. At very low speeds the curve is hardly distinguishable from the rigid wing curve; thereafter, however, the rolling power achieved by the deformable wing is much in defect of that given by the rigid wing; and especially is this so in the sonic range, where only a very small response to aileron is indicated in the particular case chosen.

We may remark that critical reversal speeds are theoretically determined in the "incompressible" range by a given value of V^2; in the subsonic range by a given value of $V^2/(1-M^2)^{\frac{1}{2}}$, or, what is equivalent, a given value of $M^2/(1-M^2)^{\frac{1}{2}}$; and in the supersonic range by a given value of $M^2/(M^2-1)^{\frac{1}{2}}$. This means that, for a stiff enough wing, there will theoretically be one reversal speed in the subsonic range and two in the supersonic, a maximum value of the supersonic rolling power (in terms of that of the rigid wing) being achieved at $M=\sqrt{2}$: the dotted lines of Fig. 2 indicate these trends. For a less stiff wing there will

* The use of M as abscissa is purely a matter of convenience; the abscissa really represents the forward speed V.

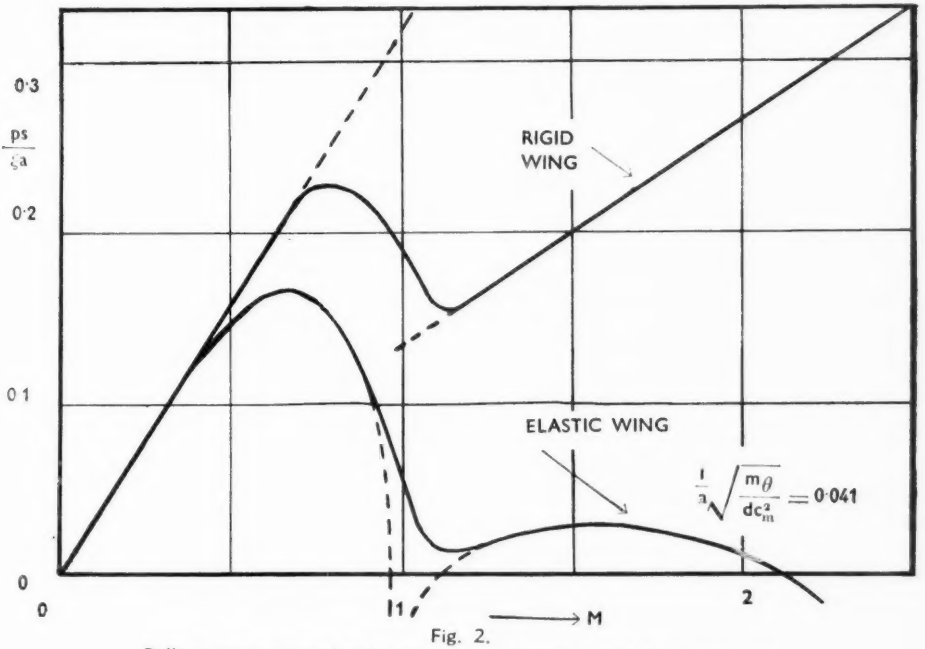

Fig. 2.
Rolling power of rigid and elastic wings: Variation with forward speed.

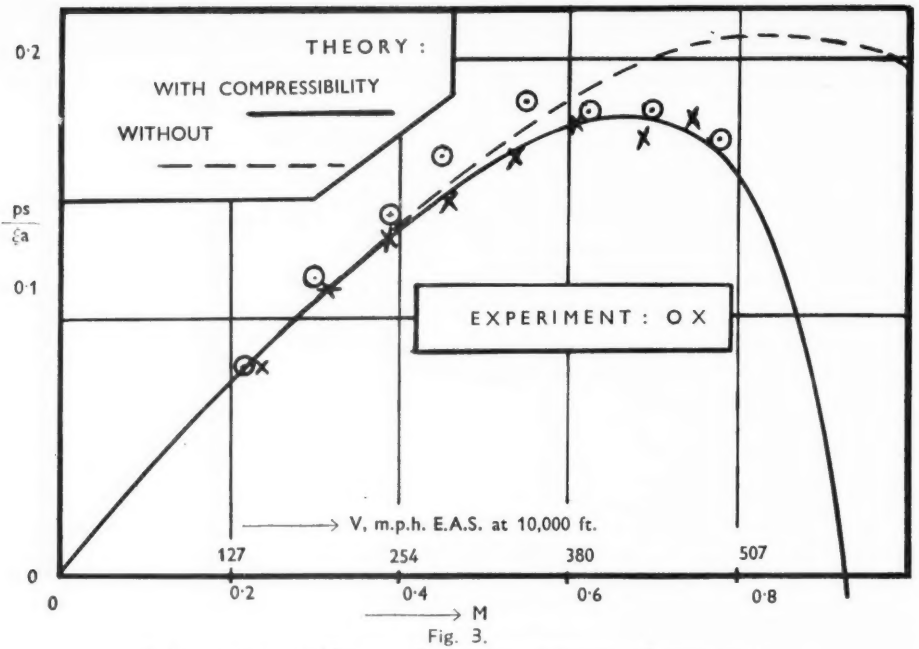

Fig. 3.
Rolling velocity of Mustang: Comparison of theory and experiment.

be no positive rolling power in the supersonic range. In practice, the large forces obtaining theoretically near $M=1$ will probably not exist, and positive control throughout the full range to final supersonic reversal may therefore be possible, as indicated by Fig. 2, provided the torsional stiffness of the wing is high enough.

To give an indication of the stiffness assumed for the typical wing of Fig. 2, we may remark that the value of the torsional stiffness criterion, calculated on the assumption that the diving speed is the speed of sound at sea level, has the value

$$(1/a)(m_\theta/dc^2{}_m)^{\frac{1}{2}}=0{\cdot}041.$$

Moreover, if the value of this criterion were reduced by ten per cent, the whole of the curve above $M=1$ would fall below the zero line, *i.e.* there would be no positive control at supersonic speed. To brighten the picture, however, we must remark that the stiffness required for positive control at a given Mach number falls off quite rapidly with altitude, being indeed proportional to the value of ρa^2. This means that if flight at supersonic speeds is restricted to heights above 30,000 ft., rolling power considerations would require only about one-third of the wing stiffness which would be necessary at ground level.

Fig. 3 gives some experimental confirmation of this method of calculation, for Mach numbers up to $M=0{\cdot}78$. The full curve is that calculated[10] for a Mustang aircraft; for comparison a dotted curve is given based on incompressible flow. The isolated points are experimental determinations of the rolling power at different Mach numbers. It will be seen that very satisfactory agreement with the first curve is achieved; and this theoretical curve, based on Glauert's compressibility correction, is thus lent support by these results.

One final point is relevant. Not only is the rolling power at supersonic speeds far lower than would be expected from subsonic

results: the hinge moments on the ailerons are also greatly increased,[9] due to the fact that the whole of the load changes are carried by the ailerons. This suggests strongly that for supersonic speeds, power-operated large chord ailerons, or even rotating wing tips, may be required.

3. DISTORTION EFFECTS ON LONGITUDINAL CONTROL AND STABILITY

As a second illustration of aeroelastic problems at high speed, we may turn to the important effects of distortion of the fuselage and tail unit of a conventional aircraft on longitudinal control and stability. In this problem many more parameters are involved than in the problem of lateral control; and to keep our discussion as simple as possible we shall therefore avoid all mention of compressibility effects. Even with this major simplification, however, the problem is still a complicated one. We have several important modes of deformation to consider: for example, tailplane twist and rotation, fuselage bending, elevator twist, and panel deformation; while these deformations may result from tailplane lift or tailplane pitching moment, or from the pressure distribution over the elevator. Finally, all these factors may make individual contributions to any of the three important phases of the subject, namely, loss of longitudinal control, structural deformation, and aircraft stability.

For our present purpose, therefore, we shall follow the precedent of R. & M. 2010[12] and shall restrict ourselves in the main to a study of tailplane deformation, examining the effects of a few of the individual parameters one at a time.

Consider first the effect of tailplane distortion on longitudinal control. Suppose that at a given speed the elevator angle is changed by the pilot. If the aircraft were rigid this would produce a given change in tailplane load and tailplane pitching moment; the load change would have a significant effect on the response of the aircraft, and the tailplane pitching moment a relatively insignificant effect. In the case of a deformable structure, however, the tailplane pitching moment produces torsion of the tailplane, in the sense which reduces the load change and hence the effectiveness of the elevator. Not only may torsion result; we have learned that in German aircraft with variable incidence tailplanes, owing to flexibility in the incidence-changing mechanism, serious effects traceable to overall rotation of the tailplane occurred.[13]

This torsion and rotation may in part be due to the change in normal load, depending on the position of the effective flexural axis; but the main effect seems to result from the pitching moment on the tailplane. This pitching moment increases continuously with speed, and with it the twist of the tailplane, so that ultimately a critical reversal speed

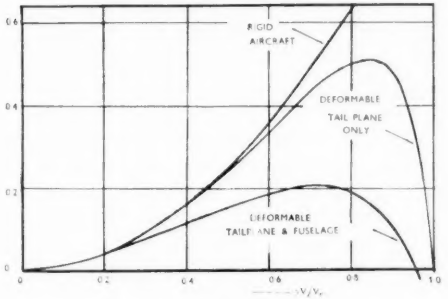

Fig. 4.
Comparative pitching moments as function of forward speed.

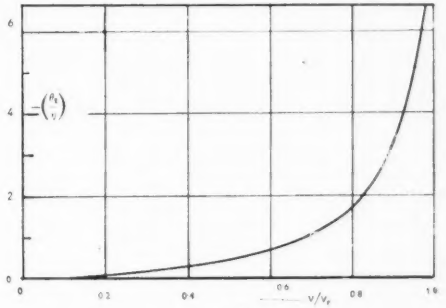

Fig. 5.
Tailplane twist per unit elevator angle: Variation with forward speed.

is reached, when the decrement of lift due to tailplane twist just cancels the increment due to application of elevator. Fig. 4 shows how the aircraft pitching moment due to unit elevator angle varies with speed in a particular case: a corresponding curve shows the pitching moment due to a rigid tailplane.

The Figure also shows the large effect resulting from fuselage flexibility. Even at half the reversal speed, the amount of control obtainable in practice is only about half that which would result if both fuselage and tailplane were rigid. One point is worth noting: although the fuselage flexibility reduces the available control very markedly throughout the speed range, yet it does not appreciably alter the reversal speed; this is of course due to the fact that at reversal there is no change in normal load on the tailplane, and the only fuselage distortion occurring is the small amount resulting from the pure moment applied through the tailplane attachment points.

So much for the flexibility effects of some components on longitudinal control. We now turn to an examination of the magnitude of the structural deformation resulting from application of elevator. By way of establishing a standard of comparison, we may note that in the case of aileron reversal, the wing deformation resulting from application of aileron is negligible from the strength viewpoint: from the maximum aileron angle likely to be applied near the reversal speed only a degree or so of wing twist would usually result. In the case of the tailplane, however, we find that very serious distortion may occur. Fig. 5 shows, for the system analysed in Ref. 12, the amount of tailplane twist (θ_t, measured at the tip) resulting from application of unit elevator angle η, and it will be seen that as the reversal speed is approached the tailplane twist increases with remarkable rapidity, until at reversal it is eight times the applied elevator angle.

This somewhat surprising result is due to the large fraction of the tailplane-elevator area occupied by the elevator (in comparison with the wing-aileron case) and to the fact that the elevator spans the tailplane and moves as a whole, while the tailplane twist is graduated from root to tip.

Figure 5 makes one thing crystal clear: we cannot permit aircraft to approach anywhere near the elevator reversal speed, as is possible (from the structural point of view at least) in the case of aileron reversal. For near the reversal speed it would be necessary to apply large elevator angles to secure the appropriate response, and the result would very probably be failure of the tailplane. It is this consideration, more than any other, which has determined[14] the value assigned to the requirement for torsional stiffness of tailplanes.

Now let us enquire how longitudinal stability can be affected by tailplane twist; for the sake of simplicity we consider only static stability, stick fixed. To understand the phenomena we must first be clear on the subject of the assessment of this aspect of stability in the case of the rigid aeroplane.

The engineering conception of stability is in essence this: if a system in equilibrium is given a small displacement, it is stable if it tends to return to the equilibrium configuration. In the case of longitudinal static stability of an aircraft, the disturbance will be a change in incidence of the aircraft accompanied by a speed change, the lift remaining constant. If therefore our aircraft is stable, stick fixed, a small increase in incidence (the elevator angle being kept constant) will produce a nose-down pitching moment about the C.G., tending to restore the original incidence. But it is not convenient to allow the response to occur if we wish to measure the stability: instead, we create a new equilibrium position, in the disturbed configuration, by moving the elevator up and so destroying the nose-down pitching moment. Thus we have the somewhat paradoxical result that the measure of stick fixed stability is the amount of elevator movement.

It is easily shown that the amount of elevator movement required to counter a

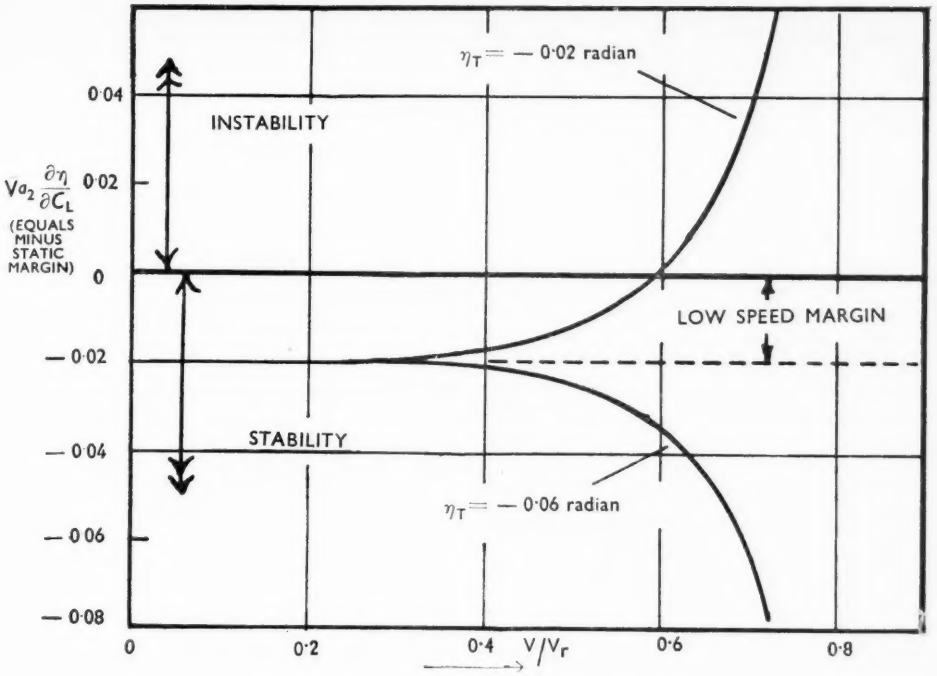

Fig. 6.
Stability variation with speed: Effect of tail setting.

given disturbance is proportional to the distance between the aircraft C.G. and a point which, for a rigid aircraft on the glide, is fixed in the aircraft—the neutral point. This distance therefore represents the margin of stability and we arrive at the concept of a static margin of fixed magnitude.

In the case of the rigid aeroplane there are three main variables entering into the definition of the static margin: the change of tailplane incidence and speed corresponding to the disturbance, and the change of elevator angle required to create the new equilibrium position. In the case of a deformable tailplane, however, we must add a fourth variable: tailplane twist. We now find that parameters such as the wing C_{mo} are brought into the picture, and in particular the tailplane setting becomes very important in the determination of the stability.[12] At one particular critical setting, the twist due to the change in speed and incidence is just balanced (theoretically) by the opposite twist due to elevator angle change; the tailplane then behaves as if rigid and the stability margin is constant. But if the setting is greater or less than the critical angle, one or other of the above factors predominates, and the aircraft becomes more or less stable as the speed increases.

These tendencies are illustrated in Fig. 6. It is to be noted that a negative ordinate implies stability, for on a stable aeroplane a positive increase in incidence requires a negative (up) elevator movement. Two curves are shown: in the lower, the aircraft is tending to become infinitely stable as the elevator reversal speed is approached, while in the upper curve it is tending to become infinitely unstable. Yet the difference in tailplane setting is only just over two degrees.

Figure 6, in effect, plots the rate of change of elevator angle with incidence against speed. In order to present the results in a

form more commonly used nowadays, I have replotted the figure in the form of elevator angle to trim against incidence (or C_L). The results are given in Fig. 7; the rapidly increasing stability with speed is shown in the upper curve by the steepening negative gradient at small incidence, and the change to instability in the other case by the change to a positive slope.

From the last Figure it is a simple matter to derive the corresponding tailplane twist as a function of speed, and this is done in Fig. 8, where the twists for the two settings are plotted against V/V_r; they become infinite at $V=V_r$. For qualitative comparison with this Figure, I have also given in Fig. 9 some experimental results recently obtained on a Mosquito aircraft by Bisgood and Lyons and given in R.A.E. Report Aero. 2125.[15] It is not possible to give a quantitative comparison, nor is it proper that I should attempt to do so here, since the Report is of an interim nature and the ultimate findings of the authors are not yet published. But the general agreement is clear.

The Report Aero. 2125[15] deserves careful study. When the ingenious experimental technique had been developed measurements were made, *inter alia*, of tailplane twist, elevator twist, fuselage bending, and panel deformation, and significant distortions of all the components at high speed were found. Moreover, the interactions of the distortions have important implications: for example, a destabilising fuselage bending can compete with a stabilising tailplane twist, and the destabilising effect can ultimately predominate. There is food for thought here: it is possible to visualise an aircraft having insufficient stiffness made easy to handle by appropriate tailplane setting, but at the cost of unsuspected structural distortion, of a magnitude which might well be dangerous.

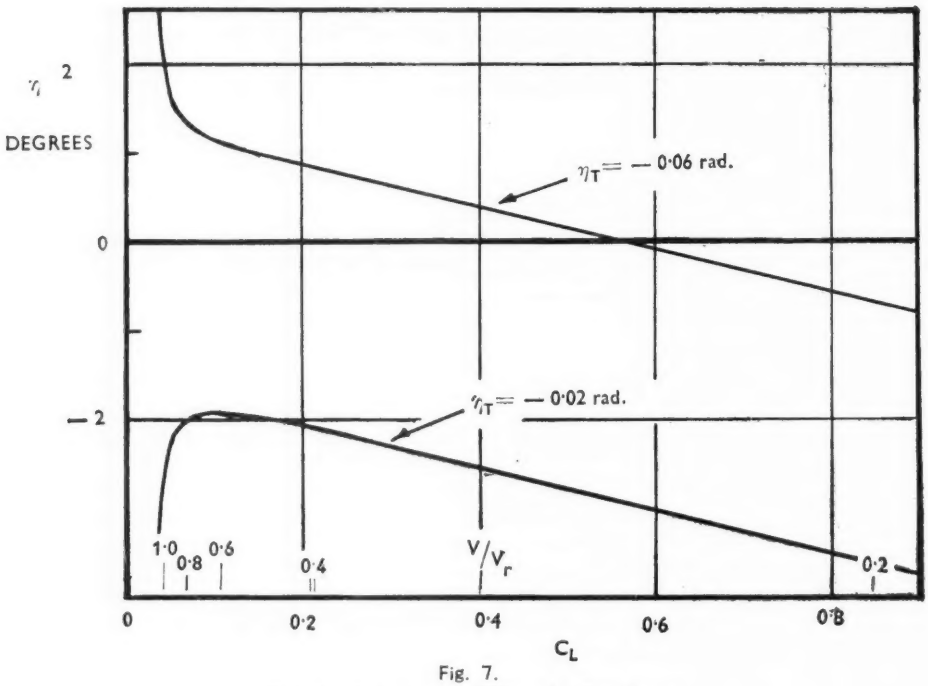

Fig. 7.
Elevator angle to trim: Effect of tail setting.

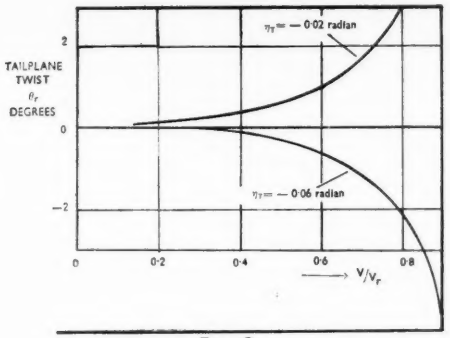

Fig. 8.
Variation of tailplane twist with speed and tail setting.

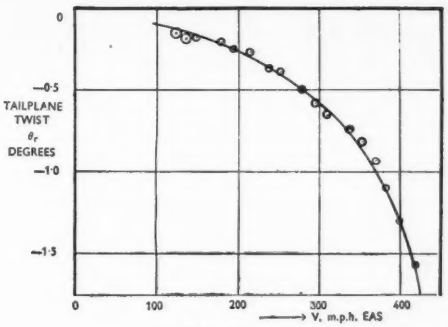

Fig. 9.
Tailplane twist on Mosquito.

I hope I have said enough to show clearly that aeroelastic effects on aeroplane stability at high speed present a problem of the first magnitude and the first importance. But I cannot leave the subject without paying tribute to the work in this field of Gates and Miss Lyon, and their collaborators. The separation by Gates of longitudinal stability considerations into static stability[16] and manoeuvrability,[17] with stick fixed and free in each case, has rendered the subject very much more tractable; and his comprehensive survey with Miss Lyon[18] of the effects of deformation and compressibility has provided a complete general framework within which all the details of the subject[19] may be built up.

4 FLUTTER

Our next illustration is in the flutter field. Flutter is probably the oldest recognised aeroelastic phenomenon, as well as potentially the most destructive, and a few words on its position in the aeroelastic scheme of things may therefore not be out of place.

As I have indicated earlier, aeroelasticity is, in nearly all its manifestations, a stability and control phenomenon, and flutter is a particular form of instability. But whereas other stability phenomena are present to a greater or less extent at all speeds, it is necessary to avoid flutter all the time. In the ordinary way, stability and control are closely bound up: for an aircraft to be controllable, it must not be too stable, but neither should it be unstable; and the motions resulting from deliberate control movements of the pilot, as well as from random disturbances, are all part of the stability picture. But even if instability does for some reason appear, it is usually within the control of the pilot; and the study of stability and control (in the usual connotation) is the study of the harmonisation of effects in a phenomenon which is present at all speeds.

With flutter, on the other hand, we are faced with a phenomenon initiated by random disturbances, which at certain speeds may appear at such a frequency that control by the pilot is quite impossible; and it may grow in amplitude with great rapidity, so that structural failure quickly occurs, sometimes in a fraction of a second. Evidently, such a phenomenon must be avoided at all costs; and for this reason a very large amount of time and energy has been devoted to study of the phenomenon throughout the world for many years.

The curative measures adopted are such that, while in some cases complete freedom from flutter can be ensured, in other cases it is relegated to a higher speed than the aircraft can achieve; and in consequence, when it occurs, it is nearly always a high-speed phenomenon. But in spite of the

almost universal respect accorded to flutter by the adoption of the latest anti-flutter measures, and in spite of the research which has been continuously carried out, flutter is still very much with us. During the war years there have been flutter occurrences on many types of aircraft, and all kinds of control surface and tab flutter have been recorded. We have learned of the importance of the virtual inertia of the air in its effect on the flutter of round-nosed control surfaces;[20] we have discovered that the positioning of mass-balance weights can be very important, particularly in spring tab flutter;[21] we have been reminded of the desirability of distributed mass balance and of the direct attachment to the control surface of the balance masses, as distinct from operation through a linkage.[22] Perhaps most important of all, we have been forced to recognise that control surface flutter can no longer be coupled with some isolated deformation of a component, but that it is associated with deformation of the aircraft as a whole.[23] All these lessons have been learned as a consequence of actual occurrences of flutter; they have brought in their train considerable researches and developments in methods of analysis, and have engendered new methods of resonance testing and much wind tunnel work.

In control surface and tab flutter, then, we have a subject rich in experience and research, from which one could well choose an illustration for a problem in high speed aeroelasticity. But I propose to turn to another flutter problem, no less rich in research, but of which there are fortunately no full-scale occurrences on record,* namely, wing flexure-torsion flutter.

I do this with, I think, good reason. The fundamentals of control surface flutter are generally understood, and though much detailed work remains to be done before accurate quantitative prediction of critical flutter speeds can be made—particularly if compressibility effects are involved—yet the essential curative measures are known. Though we could not be confident of predicting a critical flutter speed for a control surface within twenty per cent of the true value in the present state of knowledge, yet we can place a much greater degree of confidence in a calculation of the amount of mass balance required for flutter prevention: we know that if we mass-balance the control surface, paying due attention to elastic couplings, then the remaining aerodynamic coupling is invariably too weak to maintain a flutter condition. And I see no reason to suppose that the aerodynamic coupling will be strengthened by compressibility effects to a point at which a mass-balanced surface will flutter.† But though I am a thorough-going supporter of mass-balance, I will admit that it is perhaps as well for our peace of mind that we are not likely to rely on mass-balance alone as a flutter preventive when we penetrate into the sonic and supersonic speed ranges. As indicated in our discussion of rolling power, the much greater hinge moments and much smaller response in these ranges are likely to force power operation on us, and we can then rely on irreversibility of control for prevention of control surface flutter.

But there remains the question-mark of flexure-torsion wing flutter, looming larger as the unit Mach number in speed is approached. For, to avoid flexure-torsion flutter, we have in practice always relied on stiffness and not mass-balance, since we could always stave off the phenomenon by increasing wing stiffness at a much lower weight cost than that demanded by wing mass-balance.[24] But now that we are crowding close on the heels of the sound impulse as it speeds through the air, we can no longer be sure that we have staved off the trouble: our calculations are suspect since our knowledge of the air forces is but

* With the possible exception of some early occurrences on gliders, which were, however, never fully recorded or analysed.

† That is, participate in flutter of a destructive nature. Limited oscillations might possibly occur.

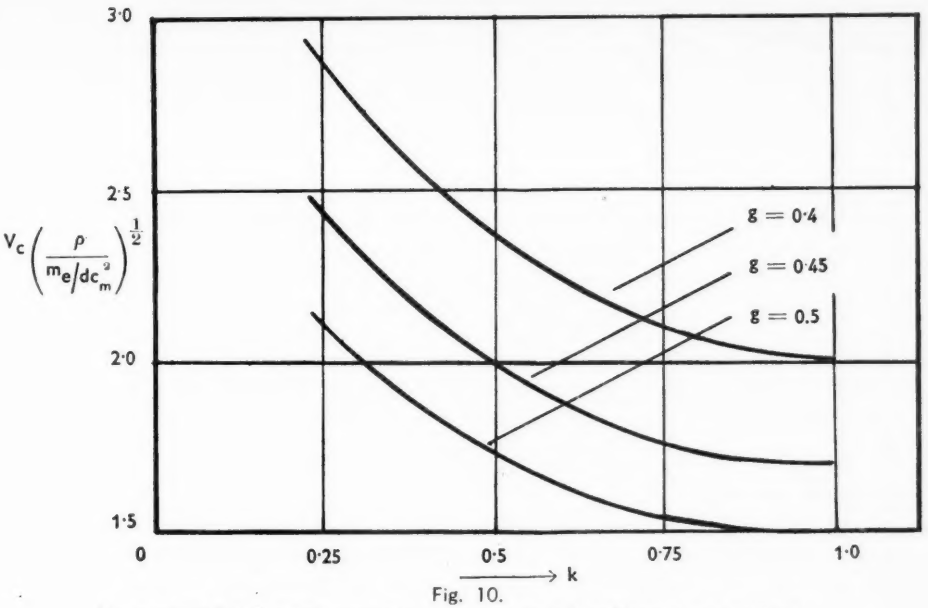

Fig. 10.

Variation of flutter speed parameter with taper ratio k and inertia axis position g.

slight, and we know how margins of safety tend to narrow at very high speeds.

Here then is a major problem, to which the complete solution is not known. All I can do here is to indicate what parts of the problem have been dealt with, and draw some tentative inferences therefrom.

First, the low speed ("incompressible") case. Much work has been done over many years on wing flexure-torsion flutter; theoretical researches taking into consideration more and more of the relevant parameters have been carried through, and these have been correlated with wind tunnel tests on models which have yielded data on critical flutter speeds and on aerodynamic derivatives. All this work has been welded into a coherent whole, so that we are reasonably confident that we can predict the flutter characteristics of a wing with sufficient accuracy provided compressibility effects are absent. The results have been given practical effect in the promulgation of the well-known torsional stiffness criterion for wings, which has been used with conspicuous success for many years.

Next we turn to the effects of compressibility at subsonic speeds. Very little attention, relatively, has been given to this problem. We have no experimental researches to guide us in our assessment of aerodynamic derivatives; and very little theoretical work has been carried out. What has been done depends on somewhat arbitrary assumptions, and is restricted to speeds at which shock waves are absent.[25]

Nevertheless, we have had to base our anti-flutter requirements on this slender foundation. As speeds have increased, and as wings have grown denser and the distribution of mass has changed, it has become necessary to modify our existing stiffness criterion, by making the value to be achieved dependent on the taper, the position of the inertia axis, and the Mach number. The Mach number correction is simply dealt with: the research mentioned above[25] showed that it is possible in some circumstances for the critical speed calculated on an incompressible basis to be reduced by an amount corresponding to the Glauert factor: that is, the calculated "incom-

pressible" speed is multiplied by $(1-M^2)^{\frac{1}{2}}$. This correction has been applied to the criterion, but limited to Mach numbers less than 0·8, since above this the theory cannot be expected to hold. For higher Mach numbers $M=0{\cdot}8$ is used in the correction.

The new criterion is described fully in R.A.E. Report S.M.E. 3357.[26] This contains a review of some previous relevant flutter calculations and a new investigation which makes some allowance for all the important parameters, in particular the difference between static and dynamic stiffnesses. The criterion gives quantitative effect not only to the Mach number correction, but also to the results that increasing taper is beneficial while rearward movement of the inertia axis has a strong adverse effect. These tendencies are indicated in Fig. 10, which shows the results of a calculation for a straight tapered wing with typical density and distribution of elasticity. The ordinate is the quantity $(m_\theta/\rho V_c^2 dc_m^2)^{-\frac{1}{2}}$, where V_c is the critical speed: it bears an obvious relation to the torsional stiffness criterion. The abscissa is the taper ratio k (tip chord/root chord) and the inertia axis is located at a fraction g of the chord aft of the leading edge.

We now come to sonic and supersonic speeds. For sonic speeds we have unfortunately to record a completely blank page: we have neither experiment nor theory on which to base any investigation. Our only hope is that if we can secure immunity from flutter in both the subsonic and supersonic ranges, then there is a reasonable probability that we shall avoid trouble in the sonic range. In the supersonic field we have some theoretical values for the relevant aerodynamic derivatives,[27/28] and it is to be hoped that some experimental check will be provided in the near future, in order to give us confidence in the theoretical results. At high Mach numbers and for low values of the frequency parameter $(2\pi fc/V)$ the derivatives have the same signs and the same order of magnitude as their low-speed counterparts, so that the flutter problem is similar.[27] Moreover, the problem of flutter avoidance is much eased by the great probability that the inertia, elastic, and aerodynamic axes will be located close together near the half-chord position; and it may well be that this fortuitous mass-balancing of the wing will in practice prevent flexure-torsion flutter for all supersonic speeds. But there are flaws in this rosy picture, among which is the prediction[28] that for Mach numbers between 1 and $\sqrt{2}$ and for low frequency parameters the aerodynamic torsional damping may become negative and so promote flutter in one degree of freedom. If this prediction is correct, we shall prevent flutter only by the provision of torsional stiffnesses high enough to ensure that the frequency parameter lies outside the critical range.

Calculations are being made of the flutter characteristics of a typical wing at supersonic speeds, and a number of interesting results have already been obtained: notably that the wing bending stiffness appears to be relatively more important than at subsonic speeds.

Let us then look back and take stock of the flutter position. For high-speed aircraft with conventional control operation, such as we now build, flutter (if it occurs) will probably be associated with control surfaces or tabs, and will be due to a lack of understanding of the details of the problem rather than the broad principles: flexure-torsion wing flutter we may hope to avoid by the methods which have succeeded in the past. But for close approach to the sonic speed and for excursions into supersonic speeds the control surface and tab problem may well disappear as a result of adoption of irreversible power-operated controls;* we shall be left with the flexure-torsion problem for these speeds. And here it seems that high torsional stiffness is the only preventive; but the standards to be achieved are not known as yet. The evaluation of these standards is perhaps the major problem in the flutter field at present:

with the ever-present dearth of experimental data on aerodynamic forces at high speeds providing a large part of that problem.

5. AEROELASTIC PROBLEMS OF SWEEPBACK

Our previous three illustrations have referred to different aeroelastic phenomena, and have been related for simplicity to the conventional aircraft. I want to give, as my last illustration of aeroelastic problems at high speed, a short description of some aeroelastic considerations entering into the design of sweptback wings[29]—a problem which is to-day of very considerable interest and importance.

It may be as well to emphasise here that I am thinking of the use of sweepback for high speeds only, and not in its relation to the development of tailless aircraft—flying wings—for relatively low speeds. I am not therefore postulating the absence (or presence) of a tail; but I shall nevertheless restrict the discussion to the effects of wing elasticity. I must also firmly disclaim any intention of trying to discriminate between the various possible forms of sweepback: whether or not the delta wing is a better compromise than the swept wing of normal aspect ratio, with or without tail, must await the verdict of the appropriate researches.

My purpose is simply to draw attention to some special aeroelastic problems associated with sweepback. It is, however, not irrelevant to say a few words on the fundamental idea underlying the use of sweepback for high speeds, since it leads to what is perhaps the most logical approach to the aeroelastic aspect.

It is well known that the object of sweepback is to delay the critical drag rise associated with Mach numbers approaching unity; and that if the angle of sweepback is β, the effective Mach number approximates (theoretically at least) to $M \cos \beta$ when the true Mach number is M. This result is made obvious immediately if we imagine an observer operating a balance which measures the forces on part of an aerofoil of infinite span: we imagine aerofoil and observer to be stationary and the aerofoil to be immersed in a stream of air of speed V normal to the span (Fig. 11A): the direction of the stream being made known to the observer by, for example, a yawmeter. In this condition, he measures certain forces. Now, however, observer, balance and aero-

* It is scarcely necessary to add that rigorous attention to the engineering problem of avoiding backlash will be required.

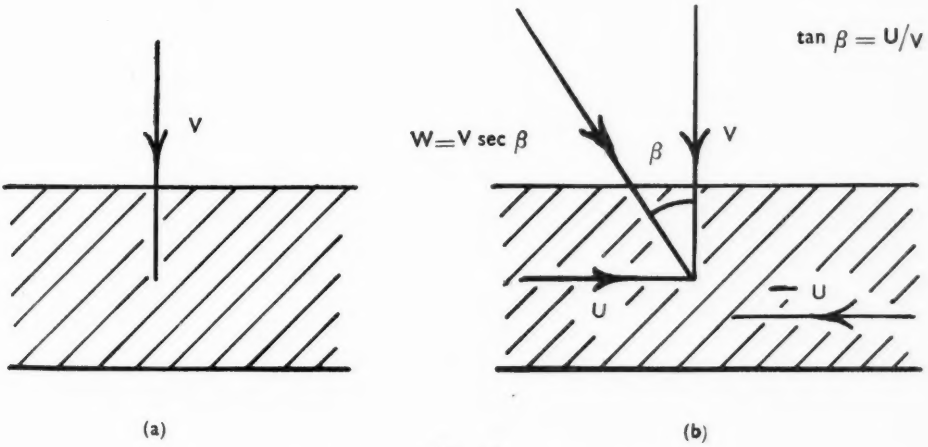

Fig. 11.
Effects of sweepback: Plan views of part of an infinite aerofoil.

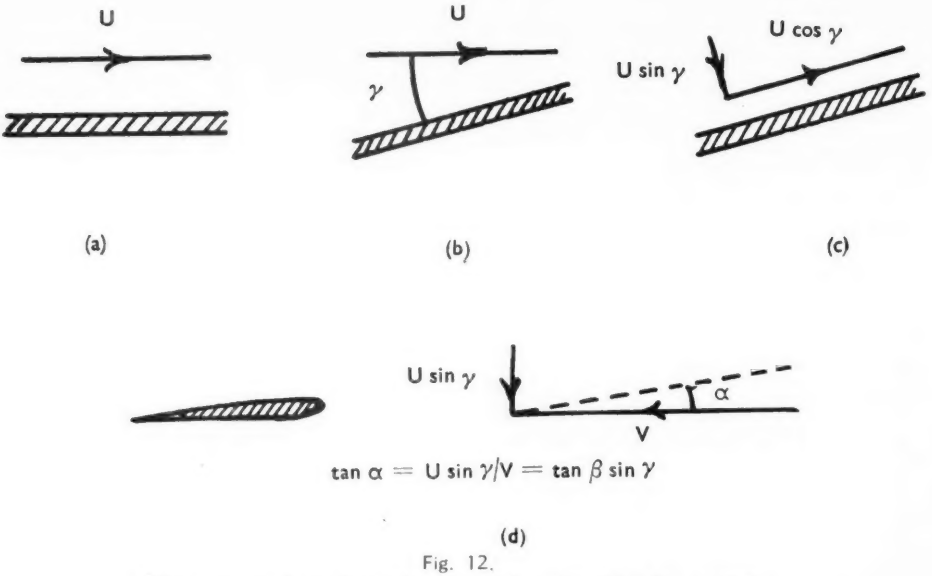

Fig. 12.
Effects of sweepback: Front elevations and section of bending aerofoil.

foil are all given a velocity $-U$ parallel to the span of the aerofoil. To the observer, this means simply that the airstream has acquired a component $+U$ relative to the aerofoil; and if $\tan \beta = U/V$, he finds the aerofoil to be sweptback through the angle β relative to a stream of speed $W = V \sec \beta$ (Fig. 11B). But, apart from some effects in the boundary layer, the flow round the section and the forces on the aerofoil are unchanged; and the observer therefore records that the forces obtaining in two dimensions are those due to the component of velocity normal to the span only. This applies, as a particular case, when the actual speed relative to the aerofoil is in the sonic region; and though the actual speed W may represent a Mach number above the critical for the aerofoil, the normal component $V = W \cos \beta$ will be below the critical Mach number if the sweepback is sufficiently pronounced.

In three dimensions—the finite aerofoil—the result is of course modified: tip effects and effects at the wing junctions have to be considered. But there remains an appreciable reduction of the effective Mach number: for normal aspect ratios, it appears that if the true Mach number is M the effective Mach number may be about $M(\cos \beta)^{\frac{1}{2}}$, instead of the two-dimensional $M \cos \beta$.

Now let us enquire into the effects of distortion. Wing twist, evidently, can be examined by a consideration of the change of incidence it produces relative to the velocity component perpendicular to the span, and no special problem is presented. But wing bending does introduce a special problem.

Let us now imagine Fig. 11B to represent part of a finite aerofoil, and let us look in Fig. 12A along the direction of the component V. The airstream has the component U parallel to the span. Now let the wing bend, the slope at the section considered changing by γ. The component U is now inclined to the span by γ (Fig. 12B), and we may resolve U itself into components along and normal to the span. If γ is not large, the component $U \cos \gamma$ parallel to the span will have (Fig. 12C) much the same effect as the original component U (Fig. 12A), and

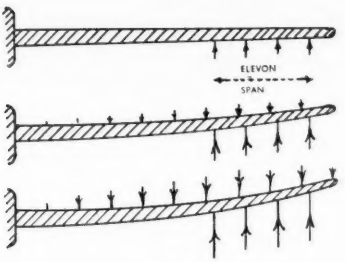

Fig. 13.
Load distribution due to bending of swept-back wing as speed increases.

may be neglected in determining the force change due to bending. But there remains the component $U \sin \gamma$, perpendicular to the span and to the component V: if in Fig. 12D we look along the span (locally) we see that the incident stream has two components V and $U \sin \gamma$ giving an effective incidence change α where $\tan \alpha$ is $(U \sin \gamma)/V$ or is $\tan \beta \sin \gamma$.*

When the wing is unswept, $\tan \beta$ vanishes and with it the incidence change: and we have the usual result that wing bending is unimportant in aeroelastic problems for the conventional aircraft.† On the other hand, we have a new and important effect to consider for the swept wing. There is in fact yet another effect: just as there is a component of slope along the wind direction, giving an effective incidence change, so there is a component of curvature, giving an effective camber change. The latter effect is obviously dependent on the details of the wing structure, being influenced for example by the direction of the ribs and their stiffness relative to that of the wing structure; its overall effect is, however, very much less than that due to the change of slope, and we shall confine our attention to the latter.

We have, then, the complication that in aileron reversal we must consider both wing torsion and wing bending. Both deformations have always been effective in the flutter problem, but there are now additional aerodynamic couplings to consider. Finally, owing to the redistribution of aerodynamic load it implies, wing bending becomes an important factor in longitudinal stability. Let us examine these effects one by one.

First, let us consider the effect of bending of a swept wing on aileron or elevon reversal; and to begin with let us imagine the wing to be infinitely stiff in torsion. Downward displacement of the elevon produces an increment of lift over the wing sections containing the elevon: at very low speeds, the force will be hardly sufficient to bend the wing appreciably (Fig. 13A). As the speed rises, however, the wing bends, and an effective nose-down change of incidence results; a distributed download over the whole wing is therefore added to the upload due to the elevon (Fig. 13B). As the speed rises still further, the nose-down incidence change produces a download sufficient to balance completely the rolling moment due to the elevon: thus a critical speed for reversal of lateral control, due entirely to bending, has resulted (Fig. 13C).

If we now add the effect of wing torsion, it becomes obvious that the reversal speed due to bending is lowered. In fact, analytical treatment shows that a given reversal speed prescribes a hyperbolic relation between the torsional stiffness m_θ and the bending stiffness l_θ of the kind shown diagrammatically in Fig. 14. The asymptote AA' defines the required torsional stiffness for an infinite bending stiffness l_θ. As the bending stiffness is reduced from infinity we travel down the hyperbola, and an increasing torsional stiffness is required: the point X represents positive control with a high bending stiffness and a torsional stiffness not very greatly in excess of the minimum given by AA'. Further decrease in the bending stiffness implies a rapidly increasing

*I am indebted to Mr. R. McKinnon Wood for this way of looking at the effects of bending on a swept wing.

†Except in acceleration problems, where inertia forces may result, and in lateral stability, where an angle of yaw may be introduced.

torsional stiffness (*e.g. Y*); then at a point where the bending stiffness is not much greater than that which would cause reversal of itself at the given speed (*Z*), the torsional stiffness, and the rate of change with the bending stiffness, is very high. Finally, the asymptote *BB′* corresponds to infinite torsional stiffness, and has an ordinate equal to the bending stiffness which would just cause reversal at the given speed.

Aileron or elevon reversal considerations, then, are somewhat more complicated for the swept than for the unswept wing. We now turn to the flutter problem. Here there is little to say, since the amount of research into this problem has not been large. But we have already remarked that the principal effect of sweepback is to introduce additional aerodynamic couplings; and since aerodynamic coupling is much less significant in the flutter problem than inertia coupling, it may well be that the requirements for flutter prevention on a swept wing will not be appreciably different from those for an unswept wing. One part of the flutter problem, namely divergence, is in fact eased, since with sweepback the incidence change resulting from a torsional displacement is reduced by the consequent bending, and the disturbing force is thus reduced.

Lastly, let us consider the effect of bending of a swept-back wing on longitudinal stability: here we shall consider only manoeuvrability, since this is associated with normal acceleration and therefore with load changes which will produce the most pronounced wing bending. Suppose the aircraft to be in a shallow dive at $1g$ and then to pull out at $2g$. This implies that the wing load is approximately doubled. If the aircraft were rigid, the aerodynamic centre of the load would not shift appreciably as a result of the change. But the wings bend, giving a reduction of effective incidence toward the tips, and these are therefore not taking their full share of the load. In con-

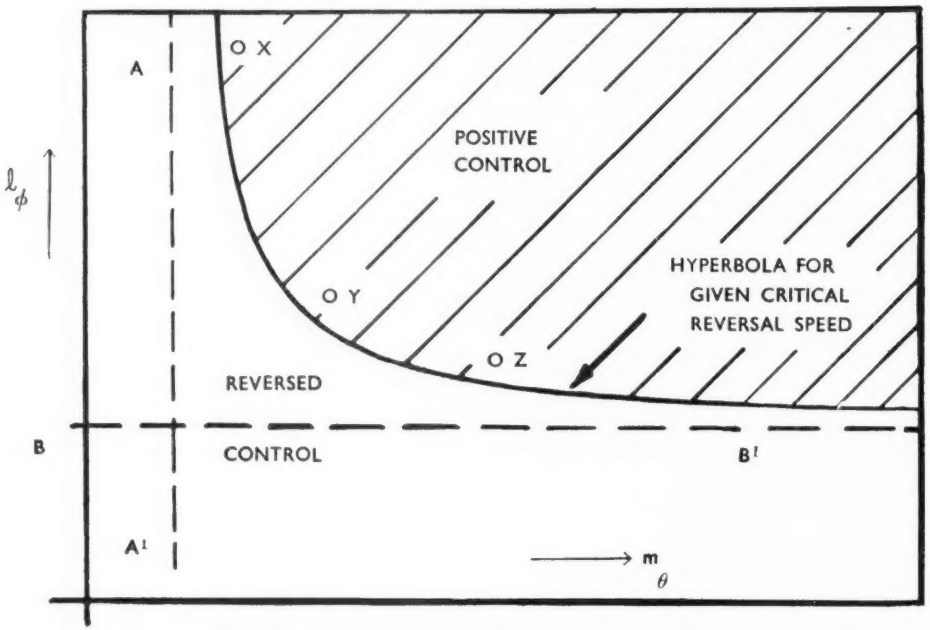

Fig. 14.
Relation between bending and torsional stiffness of swept-back wing for given reversal speed.

sequence there is an increase in the load carried by the roots and a decrease in the load carried by the tips compared with the rigid wing; with the result that the overall aerodynamic centre moves forward. Compared with the rigid aircraft, there is thus a considerable increase in the nose-up moment about the C.G., tending to tighten up the pull-out: *i.e.* the effect of wing bending is destabilising. We may remark that, towards the end of the war, this effect was causing German technicians considerable concern, and was regarded as serious even for aircraft with tails and with only moderate sweepback: they had, however, not progressed as far in analysis of the effect as in flying experience of it.[13] But analysis in this country indicates that, judged by any standard, the effect is quite serious, and considerable ingenuity will be required to overcome it. Of the three effects discussed here, this, the influence of bending of a swept wing on longitudinal stability, is probably the most important and the one demanding closest attention.

So much for one aspect of the aeroelastic problems posed by sweepback: from what has been said it is clear that the effects are understood, in the broad, and analysis of the problems is possible by existing methods, or extension of existing methods. But there are in fact two queries still to be dealt with, each in itself a problem of no small magnitude.

First, we have spoken rather glibly of the effective incidence change due to wing bending, of wing torsion, and the consequent changes in aerodynamic load. But in practice we do not know quantitatively what the incidence changes will be: the way in which a swept wing deforms under load has not been investigated fully enough. For example, the incidence change due to torsion will be affected by movement in the triangular root section, and the amount of bending will be influenced by shearing actions in the root ribs, and so forth. Moreover, even if we knew enough about the wing deformation, we should still not know enough of the resultant changes in aerodynamic load, whether at low speeds or when compressibility effects are beginning to be felt. Here then is a part of this problem calling for urgent investigation. It is my personal view that reliable data will be acquired most expeditiously by a systematic series of tests, both structural and aerodynamical, on families of elastic models of sweptback wings.

The second query presupposes that the data discussed above have been accumulated and that the bending and torsional stiffnesses required by a given design have been determined. What if these stiffnesses involve a serious penalty in weight? Can they not be reduced by the exercise of ingenuity in design? In my view, they can: though some compromise between various facets of the problem will no doubt always be necessary. For example (and I make these suggestions very tentatively, being aware that I am treading on very thin ice) all-skin designs, spoilers, decreasing sweep at wing tips, small aspect ratio and pronounced taper, and unconventional internal structure all offer possibilities worth at least passing consideration in an attack on the aeroelastic problems of sweepback.

6. CONCLUSION

In what has gone before I have tried to give illustrations of aeroelastic problems at high speed, and have also indicated the methods used and the results obtained so far in analytical attacks on the problems. To conclude my lecture, I want to make some remarks and speculations on the broad implications of the problems from the design viewpoint.

Aeroelastic phenomena, in general, involve three main kinds of force: aerodynamic, elastic, and inertial. The phenomena may therefore be influenced and brought within the designer's control by appropriate alterations to one or more of these forces. But in practice it has never been possible for a designer to alter the aerodynamic charac-

teristics of an aircraft as a control on aeroelastic phenomena: the general arrangement of the aircraft from the aerodynamic viewpoint is determined by considerations of performance and those characteristics of stability and control not essentially involving deformation. It follows that it is only through inertia and elastic forces that the designer can effect major changes in the aeroelastic characteristics. Moreover, it is only in those phases of aeroelasticity involving acceleration that changes in inertia distribution can assist the designer; and for such problems—*e.g.* flutter—the use of mass-balance has provided a most valuable means of avoiding trouble. But even for some phenomena involving acceleration—such as wing flexure-torsion flutter—it is not sufficient just to change the distribution of existing masses, though this can be very helpful; and completely to avoid trouble by alteration of the inertia characteristics would involve too great a penalty in added weight.

And so it seems that, in the future as in the past, the principal control on aeroelastic phenomena will continue to be exercised through the elastic properties of the aircraft; and the new and increasing aeroelastic phenomena will imply extensions and modifications to the familiar stiffness criterion. The extension to the wing torsional stiffness criterion has already been discussed; but changes in other criteria must also follow.

I do not intend to speculate on the forms which may be assumed by individual criteria; but one suggestion applying to all criteria is of interest. Stiffness criteria, as we now know them, may be typified by a formula such as that originally given by Roxbee Cox[30] and shown at (*a*) in Fig. 15:

$$(1/V)(m_\theta/sc^2)^{\frac{1}{2}} > C$$

where V is an equivalent airspeed in feet per second, m_θ is an elastic stiffness in pounds feet per radian, s is a spanwise and c a chordwise dimension, both in feet; C is the constant to be achieved. Now the first thing to note about the formula is that C is not non-dimensional, and the formula can therefore only be applied in the given units. Since we are contemplating changes in our criteria, it seems obviously desirable to include at the same time a change to a non-dimensional formula. Another thing: when the criterion was first formulated, the emphasis was on the permissible speed for a given stiffness; and so the speed was made to appear in the first power. Nowadays we require to know the stiffness appropriate to a given speed, and to find this we need to square our present formula. If we do this, and divide by ρ_0 (the standard air density in slugs per cubic foot) we obtain the non-dimensional formula

$$m_\theta/(\rho V^2 sc^2) > K,$$

where, by using a true airspeed V in place of an equivalent airspeed, we also write the current density ρ for ρ_0. The similarity of K to a moment coefficient is at once obvious.

We now note that the quantity K is not a constant, but depends in some cases on parameters such as taper ratio and mass distribution. In all cases, however, it depends on Mach number, and hence on velocity. It seems anomalous to introduce the speed explicitly on the left-hand side and implicitly, through the Mach number, on the right-hand side of the formula; and accordingly it may

$$\text{(a)}\quad \frac{1}{V}\sqrt{\frac{m_\theta}{sc^2}} > C$$

$$\text{(b)}\quad \frac{m_\theta}{\rho V^2 sc^2} > K$$

$$\text{(c)}\quad \frac{m_\theta}{\rho a^2 sc^2} > K(M)$$

$$\text{(d)}\quad \frac{m_\theta}{p\,s\,c^2} > K(M)$$

Fig. 15.
Possible developments in form of Stiffness Criteria.

be that we shall replace V by aM, where a is the speed of sound, and then transfer the resulting M^2 to the right-hand side to modify the dependence of K on M: thus

$$m_0/(\rho\ a^2sc^2) > K(M)$$

Finally, we may note that the criterion now involves two variables, ρ and a, where one will suffice; for ρa^2 equals γp, where γ is the constant ratio of specific heats for air, and p is the current atmospheric pressure. We thus find our stiffness criterion transformed to the unfamiliar but simple form

$$m_0/\ psc^2 > K(M)$$

where the right-hand side, while most strongly dependent on M, may also depend on other parameters.

It will be remarked that for our present designs the dependence of K on M is of the form

$$K \propto M^2(1-M^2)^{-\frac{1}{2}}\ ;$$

for supersonic designs we may expect

$$K \propto M^2(M^2-1)^{-\frac{1}{2}}\ ;$$

and it may be that one day we shall have a single formula covering adequately subsonic, sonic, and supersonic speeds. But since we are only concerned to know the minimum permissible value of m_0, a precise definition of the dependence on Mach number may not be required: all we need to know is the maximum value to be achieved by the product pK.

Much of the above is speculative; though I hope it may prove a useful line of thought for those actively concerned with the definition of stiffness requirements. But I have one more speculation on stiffness criteria: is the usefulness of the simple criterion, from the design viewpoint, coming to an end?

I do not think so; indeed, I think there may ultimately be a reversion to a simple criterion. In the early days, the great usefulness of the stiffness criterion for wings—almost its chief *raison d'être*—lay in its extreme simplicity and in the fact that a single criterion could successfully cover a multitude of possible aeroelastic sins. But we have been forced by progress in performance to abandon this position: for each criterion then we have half-a-dozen now, and the new criteria are all developing greater complexity by the inclusion of more and more parameters, in order that the weight penalty shall be minimised—even though the safety margin is minimised too. This is not all: already the question of panel deformation, where the load-displacement relation is quite non-linear, has introduced a problem in which the conventional stiffness plays no part (or at least an uncomprehended part at present); and there are stability problems which lead to requirements for stiffness different in form from those to which we are accustomed. Lastly, while now we assign individual values to the various criteria, it is already apparent that sometimes, as for the swept wing, the criteria should strictly be interdependent.

All these developments in stiffness criteria are the result of an attempt to maintain a position by which design staffs are given simple rules to follow for the avoidance of aeroelastic troubles. For some reason—possibly because it is easier to satisfy a simple criterion than to attempt a complicated analysis — aeroelasticity has been regarded in the past as the province of the few. Indeed, there has been almost a savour of black magic attaching to flutter investigations, with a few high priests who alone were initiated into its deeper mysteries. But while this position is, I am glad to say, steadily improving, we are still trying to develop criteria to be used as short cuts to the full analysis of aeroelastic phenomena on new aircraft—and this in spite of the notoriously unsatisfactory nature of short cuts in unknown country.

This is, in my view, a wrong procedure, and a use of stiffness criteria for which they were never intended. They should be used as yardsticks by which, in the early stages of design, the elastic properties required for an aircraft can be estimated without serious error, and by which the final design can be compared with other aircraft. But they

should not be used to avoid a careful and systematic analysis of aeroelastic phenomena. I think it likely therefore, that it will be found necessary for firms engaged in the design of high-speed aircraft to develop specialist groups to deal with the aeroelastic considerations, such groups working hand-in-glove with the stress offices in order that there may be a proper correlation between strength and stiffness. When that time comes we shall find stiffness criteria playing their proper part in design rather than the artificial part which is now assigned to them.

In closing, I have to acknowledge my indebtedness to the Ministry of Supply for permission to refer to unpublished papers and unreported work. I have also to record my indebtedness to the stimulating discussions, by Committees and Sub-Committees of the Aeronautical Research Council, of aeroelastic problems at high speed.

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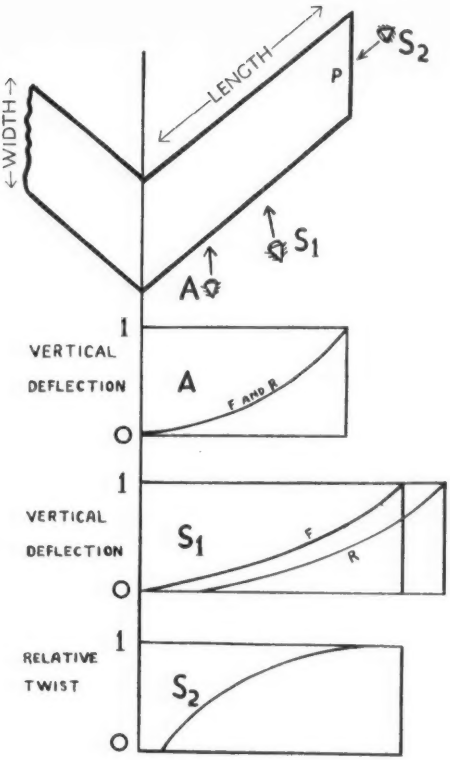
DISCUSSION

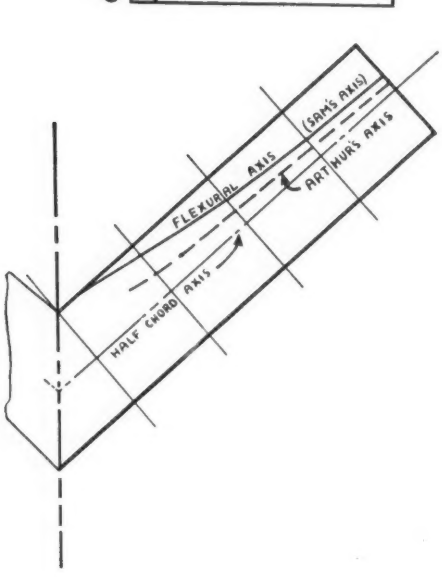
Professor G. T. R. Hill (Fellow): He confessed to considerable diffidence in opening the discussion on so masterly an exposition of the subject as had been presented. He was no high priest, and many of the high priests to whom the author had referred were present; indeed, he felt he was not even a low priest, but just a humble sweeper of the temple.

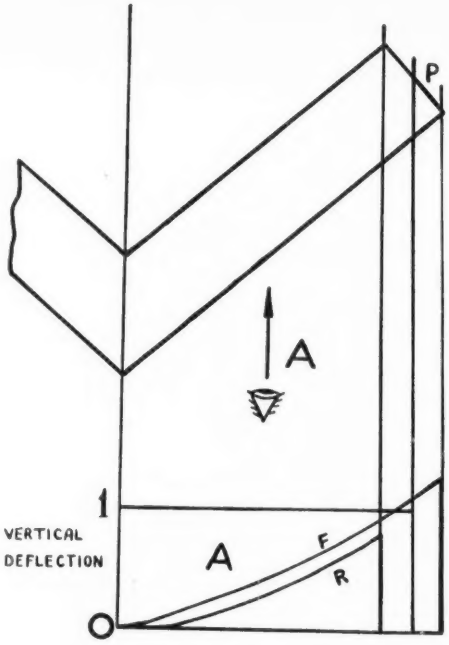
Some 12 years ago the tailless aircraft with swept-back wings known as the Mark V Pterodactyl, built at the Westland Aircraft Works, had experienced reversal of the controllers at high speed in a dive to which the author had referred. A lot of work had been done at that time on the subject of deflections of a swept-back wing by Mr. H. A. Mettam, and he regretted that that work had not been written up for the benefit of those who came afterwards.

Coming to the present, he felt sure there were some who had found the problems of actual geometrical distortion of swept-back wings very difficult to clarify in their minds; an alternative approach might sometimes be useful, particularly for engineers whose mathematics were not all that they should be. Such an alternative could perhaps be paralleled in the case of pitching moment coefficients, which were handy for calculating; but centres of pressure gave, in his view, a clearer physical idea of what was actually happening.

Illustrating that approach by means of diagrams, he introduced two imaginary friends; and he thought the difficulty arose because in the case of swept-back wings they took different points of view. He named those friends "Structural Sam" and "Aerodynamical Arthur," and illustrated their







points of view by reference to a simplified structure of two spars and two ribs.

He felt that Professor Collar was pessimistic in ruling out any possibility of aerodynamic adjustment being called in to deal with flutter problems. An approach along that line would be of very real value.

He asked for some clearing up of the nomenclature for the kind of picture he had portrayed. In order to avoid confusion, he indicated the dimensions which, he suggested, should be referred to as span, chord, length and width respectively.

He paid sincere tribute to the pilots, observers and aircrews who made tests for flutter in the air. From personal experience of flutter many years ago, in the days before parachutes were provided, he knew how terrifying flutter was.

Mr. W. G. A. Perring (Fellow): He paid tribute to Professor Collar for his masterly treatment of the aeroelastic work as it had

developed over the past five to ten years. The problem was one that concerned the detail parts of the aircraft, as well as its major components, and in all its forms it was liable to give rise to undesirable aircraft characteristics. For example, distortion of the fabric or metal covering of control surfaces had a large and important effect on the heaviness of control and could have a serious effect on the general stability and response of the aircraft.

Professor Collar had drawn attention to the effects of more general distortion of such parts as the fuselage, tailplane and elevators, but an important point to recognise was that although the consequences of these local and general distortions were often associated with, and tended to become more marked at, high speed they were not, in fact, compressibility effects but were the direct result of the elasticity of the aircraft and its parts.

In tests of the Mosquito, for example, the major part of the distortion that had been measured in flight could be accounted for by calculation of the distortion arising from the air loads in the absence of compressibility, the contribution resulting from the change in these loads due to compressibility being quite small. The fact that the aircraft could not be regarded as a rigid body was now recognised by designers and the important effect of the flexure of even such relatively stiff members as, say, the fuselage, on aircraft stability was now more generallly realised.

In considering one of the direct effects of compressibility, Professor Collar had drawn attention to the important problem of loss of control, and had outlined the problem of loss of, or reversal of, aileron control. Considerations of this kind, while demanding increased stiffness, and in particular increased torsional stiffness, would force designers in the end to think of alternative forms of control and would probably result in the acceptance of all moving wing tips and all moving tailplanes.

In some ways Professor Collar was reassuring on the question of flutter and had rather left it to the structural specialist to find the solution.

On the usefulness of criteria, he was not quite sure where Professor Collar's line of reasoning was leading. He drew attention to the part simple stiffness criteria had played in guiding aircraft design, and, he thought, was about to put forward a case for continuing this policy of basing design on generalised stiffness criteria. It was then that he put in a plea for the building up of specialist groups to deal with the aeroelastic considerations and work hand in glove with the stress office.

He wondered whether Professor Collar anticipated that there would be greater use of dynamic models and the need for more tests on specific designs in the future than in the past. If this was the case it was important and the work would demand many more men and much more test equipment than they had ever had in the past.

A policy on these lines was followed in Germany and in America, but it was one that had never been attempted in this country. Were they to infer from Professor Collar's concluding remarks that greater use would have to be made in future of models to investigate the aeroelastic problems, treating each design as a special case?

Dr. P. B. Walker (S.M.E. Department, Royal Aircraft Establishment, Fellow): Professor Collar had implied that increased stiffness was probably the only solution for reversibility of control, and Mr. Perring had suggested the all-moving control as an alternative. There was a third possibility. A control surface was originally envisaged as a device that was fitted to a rigid structure. Then they had found that the structure, as, for instance, a wing, was not really rigid, and in comparative terms it had become less and less rigid; by "comparative" he meant in relation to speed. In response they had tried to put the reversal speed higher and higher,

so that it was always higher than the maximum diving speed. What happened if they accepted flexibility, putting the reversal speed as low as possible and using elastic distortion as a primary method of control? If they did that, the aileron or the elevator became merely a kind of servo device. He did not know whether that suggestion could be taken seriously, but would like Professor Collar's views.

Mr. R. Smelt (Gas Dynamics Department, Royal Aircraft Establishment, Associate Fellow): Those who had been experimenting in high-speed flight at high Mach numbers were indebted to Professor Collar and his colleagues for their work during the war years, for without that work little could have been done on those high-speed experiments. It was a great tribute to that work that in applying it they had never really suffered any trouble.

Perhaps his real job was to apologise to Professor Collar for the fact, which had been made very clear, that one of the major difficulties of the problem was the absence of real aerodynamic data at high-speed. He could only plead that the problem was difficult, and that the pilots and the technical people had really done their best. In that connection he recalled an occasion when an attempt was being made to measure the effectiveness of the elevator tab deflection at high Mach number, where wind tunnel tests indicated only very small hinge moments from the tab. The pilot of the machine, when he had landed, had complained that he had found the aircraft practically uncontrollable in the dive, and said that he had experienced a very bad time. It was suggested to him that he had nothing to grumble about because he had got out of it quite well. He had replied that he had considered the suggestion of the technical people to be on the high side, and that he had only put on half the amount which they had suggested; an amount which wind tunnel tests had indicated to give only slight changes in stick force. That experience indicated the difficulties of obtaining control characteristics at high Mach numbers.

Quite a lot of difficulty had been found in separating out the effects of distortion and Mach number itself. That was illustrated by Fig. 7 in the paper, where the author had shown the way in which the elevator angle at low lift coefficient, at high speed, tended suddenly to change with increase of speed and decrease of C_L. The difficulty was that on most aircraft a similar effect was found due to Mach number alone, as distinct from distortion; it was extremely difficult to separate them, and the only practical way was to watch the effects of altitude. The change due to Mach number remained at high altitudes, while the effects of distortion tended to disappear as indicated speed decreased. He did not think that a really satisfactory method of separating the two effects had been arrived at, and would welcome Professor Collar's remarks on that problem.

Mr. A. N. Clifton (Vickers-Armstrongs Ltd., Fellow): He would like to consider the case of a conventional aeroplane diving at a high subsonic speed. The author had referred to the effect upon the aeroplane stability of the deformation of the fuselage and tailplane. But another effect which might be important in that connection was the twist of the main plane itself. Had account been taken of that factor?

With regard to the term "aeroelastic," a better term might be "airframe elasticity."

The Chairman: The term "aeroelasticity," if it were not his own invention, was the invention of someone very close to him. It was a portmanteau word intended to embrace the two notions of aerodynamics and elasticity.

Mr. E. G. Broadbent (S.M.E. Department, Royal Aircraft Establishment): He was interested to note that Professor Collar considered wing flutter, *i.e.*, flexural, tor-

sional flutter, to be more important than control surface flutter, explaining that control surface flutter could be cured by mass balancing and probably would not be troublesome in the future at supersonic speeds. But it seemed that, while they were preventing flutter of control surfaces, by mass-balance, they might possibly at the very high speeds experience a type of flutter of which account was not taken, namely, flutter in which the main surface concerned was not bending in the fundamental mode of vibration. Their present ideas of mass balancing applied only to the fundamental mode of vibration.

In the swept-back wing there was an aerodynamic coupling due to wing bending, and he wondered whether flutter due to flexure would arise. It seemed that it should be possible to experience wing flutter in two flexural modes alone, and therefore, a wing infinitely stiff in torsion would not necessarily be free from it.

Mr. N. E. Rowe (Vice-President, Fellow): It seemed to him that the form in which the forces arose put a premium on flying high; on the other hand, they were in a position to fly high to-day, using the gas turbine, and possibly they would want to fly even higher in the future.

One of the biggest problems to-day in civil aviation at high speed was that of gusts. The aircraft might be made to stand up to it perfectly well, but it might easily become very bad indeed for the passengers, so bad that they would not be prepared to fly in bad weather. If the effects were to be reduced in gusty weather, the speeds might have to be so reduced that jet propulsion would not be able to come into its own as they hoped it would.

The effect of G on the swept-back wing seemed to indicate that under gusty conditions the effect on aircraft passengers might be much less than would be the case where the normal wing was used. That seemed to place greater emphasis than ever on the need for the development of designs of the swept-back class of wing.

Dr. Roxbee Cox: In the early part of the paper the point had been made that the concept of semi-rigidity had to be abandoned because in effect it gave a hidden margin of stiffness, which they could not afford to have, and that more accurate and more complicated methods were necessary. Had Professor Collar and his associates ever tried developing the older method, with, say, two degrees of freedom, to see whether they could approximate more nearly to the truth, and at the same time retain the simplicity of the semi-rigid idea?

Another interesting matter was the indication that loss of aileron control due to wing twisting became a more serious problem than ever at very high speeds, and the suggested possibility of having to go to the all-moving wing tip. Had Professor Collar considered the possibility of using ailerons on the front of the wing instead of at the back? In that case, if the wing twisted, it would at least be twisting in the right direction. There was also something in the possibility of deleting the ailerons, control being achieved by a number of jets!

He wondered whether Professor Collar's calculations on wings of high Mach number were calculations on wings of conventional proportions; the wings likely to be employed at very high speeds would be of very different shape and very different aspect ratio. He wanted to be sure that the sort of wing that Professor Collar was considering was a practical one for the speeds at which they would probably fly in the future.

He supposed the swept-back wing would not necessarily be with them always, because if they ever flew well beyond the speed of sound there was no particular point in sweeping the wing back. In the author's "Conclusion" he had said that the inclusion of more and more parameters and complexities in dealing with the question of stiffness minimised the weight penalty, even

though the safety margin was minimised too; they were forced to use those methods because they could not afford the weight penalty which must be paid if they did not use them. He wondered whether the author had really meant what he had said. He hoped that they were reducing the ignorance margin; he did not think that, in any case, they could afford to reduce the safety margin.

Mr. W. S. Rope (Associate Fellow): An idea for an aeroplane which might be of interest was to have the wing hinged in two places, so that there were three sections of the wing; there were two fuselages, one attached to each outer section, and the hinges were inclined outwards and forwards, so that, as the centre portion of the wing was depressed, its incidence increased and it was conceivable that it would be stable. Each fuselage had a tailplane and elevator. The elevators were used differentially and in the opposite direction from ordinary ailerons, after the mannner of servo tabs, to cause the aeroplane to roll by twisting the whole aeroplane, and were used together as an elevator. Did Professor Collar think that such an aeroplane had possibilities?

The undercarriage should have two wheels in tandem under each fuselage and a single wheel under the nacelle in the middle; it would then be able to land on uneven ground and would have all its wheels on the ground at the same time.

Professor A. G. Pugsley, O.B.E., D.Sc. (Professor of Civil Engineering, University of Bristol, Fellow) *Contributed*: He would like to draw attention to the special place in the Society's annals of the paper presented by Professor Collar. The first lecture to the Society in this field was delivered by Roxbee Cox in 1933. Mass balancing of control surfaces was then the question of the day, but in treating this and other matters Roxbee Cox introduced his idea of statistical criteria, linked with torsional and flexural stiffnesses. The second comparable lecture, delivered by himself in 1937, was largely a development of the torsional stiffness criterion theme in relation to aileron reversal, wing flutter and wing divergence. Both papers were written with the outlook of the designer in mind; Professor Collar, with his experience of both the N.P.L. and the R.A.E., had now presented a much wider picture of interest to research workers and designers alike. Moreover, as was natural to the subject and in some degree making amends for the shortcomings of the previous papers, he had given aerodynamic considerations a larger share of attention, while very ably continuing the history of the stiffness criteria theme which was so characteristic of aeroelastic work in this country.

The discussion of Professor Collar's paper had naturally touched on areas mentioned during the discussion of the earlier papers. In particular, Dr. Walker's remarks reminded him of a diagram in his paper, around which he had discussed the idea of following "historical routes" such as EF_1, EF_2, EF_3 and EF_4.

Since that time wing densities had continued to increase and they had actually followed routes EF_1 and EF_2 (for wings without and with heavy wing engines respectively) rather than EF_3, which he took to be in Dr. Walker's mind. He felt that for high subsonic and supersonic speeds they must for the present continue to follow the high stiffness path represented by EF_1, but he would like to know Professor Collar's views on this.

Professor Collar's illuminating excursion into the realm of aeroelastic stability reminded him of an early study in this field on the effects of wing twist upon longitudinal stability (R. & M. 1548). Hitherto there had been little likelihood of stability trouble due to wing twist, but with the very high speeds and thin wings now envisaged it might be relevant to reopen the matter. His conclusion at the time was: "that for an aeroplane as at present designed wing

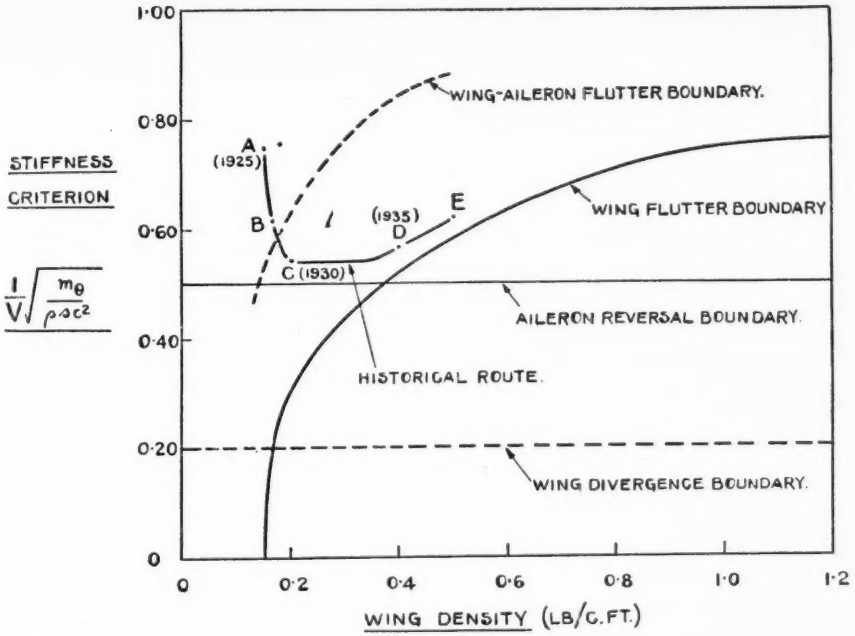


Fig. 5.

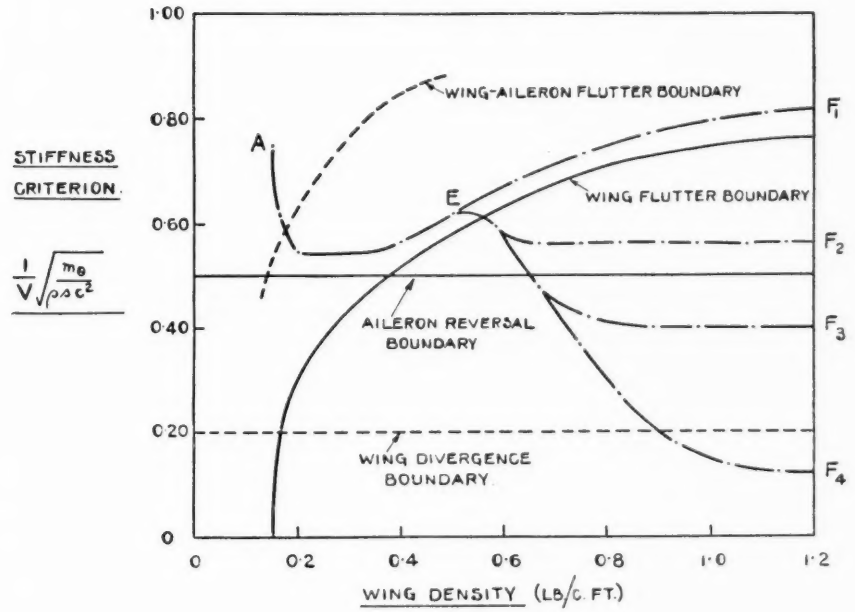


Fig. 6.

Reproduced from "Control Surface and Wing Stability Problems" by Professor A. G. Pugsley, Journal of the R.Ae.S., November 1937.

elasticity will usually have upon longitudinal stability at high speeds a detrimental effect, which will tend to be more or less dangerous according as the speed of the aeroplane is close to or remote from the divergence speed for its wings. To eliminate this effect the ideal course would be to adopt a wing section with a constant C.P. position (k_{m0} zero) located on the flexural axis of the wing. As practical measures towards the same end, k_{m0} for the wings should be made as small as other considerations may permit, and the stiffness of the wings in torsion should be large." Could Professor Collar comment on the possible relevance of this to modern high speed flight, having in mind particularly that his work envisaged only subsonic flow conditions?

He liked the $m_\theta - l_\theta$ diagram of Fig. 14, although—he supposed mainly for historical reasons—he would prefer to see m_θ plotted as ordinate rather than abscissa!

PROFESSOR COLLAR'S REPLY

Professor Hill:—He was not sure that Professor Hill had asked him any questions, although he had added to their knowledge of the subject and had indicated a new way of thinking about it. He was interested in the reference "Aerodynamic Arthur" because "Arthur" was his own first name! He had wondered who was his counterpart, "Structural Sam," but had not been able to place him.

He agreed that it would be possible to use aerodynamic design in the control of aeroelastic phenomena to some small extent; but he thought that in order to change aeroelastic phenomena at all fundamentally some considerable changes in the aerodynamic design, which would be inadmissible from the point of view of performance, would be necessary.

Mr. Perring:—Stiffness criteria had begun in Roxbee Cox's statistical analysis, and were presented then in the simplest form. But when used as design criteria, their extreme simplicity rendered them uneconomic, since so many parameters were left out of account. At first, this was hardly a defect at all; but latterly it had become serious, and it had been necessary to generalise the criteria. This was a useful step, since it led to more economic design; but even so many parameters were still left out of account, while the effects of those introduced were only approximately represented in the generalised criteria. Thus even a generalised criterion could not lead to the most economic design; only a full analysis could do that. At present, and for some time in the future, generalised criteria were likely to be a mainstay of aeroelastic design: they would fulfil a most useful function in the intervening period before staff experienced in aeroelastic matters became available in the design offices of firms. For that reason, he thought that work on generalised criteria should be actively pursued until the criteria themselves could be discarded in favour of full analysis.

These were developments in the analytical field.

He was not at all sure that the use of models for tests of specific designs, on the lines followed in Germany, was desirable in this country, having in mind the general economy of the picture. It was very nice if it could be afforded; but it was expensive in money, equipment, time and staff; moreover it had a tendency to lead to *ad hoc* cures of troubles which were imperfectly understood. With the relatively limited resources available here, it would be more profitable to put their effort into fundamental research and so to provide a better understanding of, and a better basis for analytical attacks on, the problems.

The position was slightly different in regard to the first supersonic aircraft; these were likely to need very thin wings with very

thick skins, with weight pared to an absolute minimum. For these aircraft special tests were probably desirable. To sum up, he considered that for really economic design in the future each new aircraft would eventually have to be treated from the analytical viewpoint as a special case, while sometimes special models would be required as well. Meanwhile, no doubt they would have to rely on some form of compromise; more than that, they would have to rely on the goodwill and bravery of pilots for testing things of which they were not really sure until they took the air. But he did urge that the subject was such that it deserved a great deal more attention than had been given to it in the past.

Dr. Walker:—The suggestion that they should accept flexibility and use it, was of course, a possibility. It was limited by the amount of distortion which could be permitted by strength considerations, and probably by stability (as distinct from control) effects. As was pointed out in the paper, in the case of lateral control the amount of wing distortion was at present relatively slight; that being the case, it might be that they could decide to warp their wings by the action of the ailerons. But in the case of the tailplanes he did not think that was possible unless they changed the design radically, and in particular reduced the amount of space occupied by the elevator; because once they had got to a very high speed with a conventional design they would be exceeding the 8/1 twist ratio of tailplane to elevator angle shown in Fig. 5, and he believed that the very first time they attempted such a thing they would just twist off the tailplane.

Mr. Smelt:—The lack of data in aeroelastic problems was felt, not only in the aeroelastic field, but in every other. Experimental work on a wider scale than any on which it had been attempted so far was really badly needed. They had fairly workable theories, so far as they knew; and they had a good deal of *ad hoc* but uncorrelated experience from flights in new aircraft by test pilots; but in order to tie up the somewhat dubious predictions with the actual experience, much more systematic work was desirable, partly in flight tests and partly in experimental testing on the ground.

As to the separation of distortion and compressibility effects, he could see no other solution than that of making tests over a range of altitudes.

Mr. Clifton:—In reply to the question on the effect of twist of the mainplane itself; for conventional cambers there was a destabilising effect, but he believed it was not, in fact, very important. Professor Pugsley had reported on it in R. & M. 1548 some years ago, and had concluded that it was not going to be a very serious effect. The actual amount of wing twist that might be expected from the torsional moment of the wing was probably not much greater than a degree or so—even less if appreciable upfloat of the ailerons occurred. This would mean that, to maintain constant lift, the root incidence would be increased at high speed by perhaps half a degree, and this increase would be transmitted by the fuselage to the tailplane. Compared with the other distortions he did not think it would represent a very serious contribution to instability.

As to the criticism of the term "aeroelasticity," for which the Chairman had confessed he might have been responsible, he could only say that if anybody could suggest a better term it would be welcomed. The suggested term "Airframe elasticity" was rather limited; they should embody in the term all the forces—aerodynamic, elastic, and inertia—contributing to the phenomenon.

Mr. Broadbent:—The possibility of control surface flutter with the main surface bending in an overtone mode had often been considered, but the theory and experiment agreed that the displacements in an

overtone mode were not usually sufficient to permit flutter to exist except for surfaces of very high aspect ratio. But in any case, properly distributed mass-balance for the control surface should provide a cure.

The other question with regard to flutter of a swept-back wing in two bending modes was a new one for him, and it was rather horrifying. It seemed quite a possibility. The essential thing for the promotion of flutter was that it should be possible to change the incidence of the section (of a wing, for example) and at the same time to move it up and down out of phase: in this way the necessary energy could be extracted from the air. Bending a swept-back wing did provide an effective change of incidence, and with the two bending modes it was possible for one to be out of phase with the other, and to obtain flutter from that. An examination of the magnitude of the inertia coupling between the two bending modes would, however, be necessary before any prediction could be made of the probability of flutter occurring.

Mr. Rowe:—So far as they could see it was in fact the case that sweepback would help the gust problem.

He agreed that his paper brought out the value of flying at high altitudes. Broadly speaking, aeroelastic phenomena were governed by the equivalent airspeed. Flying at a given Mach number did not imply a serious loss in true speed as the height increased, but the equivalent airspeed decreased rapidly, and with it the incidence of aeroelastic phenomena.

Dr. Roxbee Cox:—He could not remember whether he and his colleagues had seriously considered using two degrees of freedom; but it had been demonstrated quite well that the method adopted was not very much longer to use, nor was it very much more complicated, than that involving the concept of semi-rigidity with only a single degree of freedom; therefore, it did not seem to be worth while considering the intermediate step of using two degrees of freedom.

The Chairman had suggested that two degrees of freedom in a semi-rigid approach would give an answer nearer the truth than a single degree of freedom; but he must ask, like Pilate, "What is truth?" The trouble with the single degree of freedom was that it involved a hidden margin of uncertain magnitude and the truth, in the sense of an accurate stiffness requirement, was unknown. Two degrees of freedom, although undoubtedly better, still involved a margin of unknown magnitude. It was to avoid this unknown factor that the new approach had been devised.

The suggestion to use forward ailerons had been made by a number of people at various times, and he had himself suggested it very diffidently and tentatively to the people most concerned with performance; but it had been turned down in no uncertain terms. It would be extremely difficult to do mechanically, and the discontinuities and gaps near the nose would adversely affect the performance of the aircraft. There was also the fact that forward ailerons would be strongly overbalanced aerodynamically from the word "go" and, in consequence, something would have to be done to avoid negative stick forces all the time. An obvious possibility here would be to interconnect a forward overbalanced aileron to a conventional underbalanced aileron at the trailing edge; to achieve balance of a forward aileron of reasonable proportions, a very large chord conventional aileron would be required. From such a design it would be only a short step to joining the two parts together to give the all-moving wing tip he had suggested.

Since various speakers had raised various possibilities of overcoming the aileron reversal difficulty, he himself would add one, namely, the possibility of using much larger tabs than had been used in the past. The pitching moment on a wing section due to a tab was much greater, in proportion to its

size, than that due to an aileron; and it was theoretically possible to obtain an aileron with a geared tab which would have no net twisting effect on the wing. But the tab would be so large that the aileron would be overbalanced. This would rule the system out for manual operation, but might be admissible with powered controls.

He did not feel really competent to speak about the suggestion that they should do away with sweepback when they had penetrated to the supersonic region, but he believed that sweepback would not be used, and that instead they would rely on very small aspect ratio. In view of the extreme uncertainty, in the present state of knowledge, of the wing planform which would be used at supersonic speeds, he had used in his illustration of aileron reversal a conventional elliptic wing. The analysis was general, and the conclusions could be applied qualitatively at least to small aspect ratios: but there was a weakness in the carrying over from the subsonic into the supersonic range, without rigorous justification, of certain assumptions relating to independence of coefficients on aspect ratio.

By "safety margin" he had intended to imply "safety margin plus ignorance margin," and, as had been suggested, he was only concerned with reduction of the ignorance margin.

Mr. Rope:—In putting forward the possibility of using a hinged wing, with hinges inclined outwards and forwards, he had been largely anticipated by a French patent: the wing system was the same, but a single conventional fuselage was used. Firstly, it was claimed that by such a means the shock due to landing would be minimised; because, on impact, the wing tips would be displaced downwards, and in virtue of the hinge directions would thereby acquire a considerable increase of incidence and would therefore be cushioned by the air. The second claim was of a related nature, namely, that the effects of a gust, which would cause upward deflection of the wing tips, would be strongly offset. Thirdly, it was claimed that it was not necessary to fit any ailerons; rolling would be achieved by differential movement of the elevator. In that case each wing would lag slightly behind the fuselage, and there would be a rolling moment in the right direction.

An actual design had been put forward, but in his view it had involved such complexity as to make it an extremely difficult thing to realise in practice.

Professor Pugsley:—In view of his long-standing interest and pioneer work in the field of general aeroelasticity, he welcomed particularly his contribution.

Professor Pugsley had asked his opinion as to the possible paths to be followed by the value of the stiffness criterion in his historical diagrams, and what might be the impact of the compressibility effects, particularly for supersonic conditions. It was of interest to note, firstly, that at the time of his lecture, wing densities had reached 0.5 lb./cu. ft.; and his diagrams showed possible paths extending to 1.2 lb./cu. ft.; in the intervening period, however, densities had commonly risen to 1.6 lb./cu. ft. and values in excess of 2.0 lb./cu. ft. had been recorded. They had thus to extend his abscissa by some 100 per cent. to cover modern designs, and probably by much more for supersonic designs.

In addition, they had, as he indicated in his paper, to add to his "incompressible" boundaries new boundaries for the high subsonic, sonic, and supersonic speed ranges. Yet another complication was provided by extra boundaries defined by stability and distortion effects. As a result, they had a multiplicity of boundaries—those for the sonic range very ill-defined indeed—and a multiplicity of possible paths. He agreed with Professor Pugsley that it was unlikely that they could tread any path other than that of high stiffness (such as his EF_1) without crossing some important boundary and so trespassing on dangerous ground.

Professor Pugsley had also referred to his

discussion in R. & M. 1548 of the effects on longitudinal stability of wing torsional stiffness, which he had cited in his reply to Mr. Clifton. His conclusions, derived from a study of the stability equations, were of course in accord with what might be inferred from considerations of static stability and manœuvrability. If the wing C_{mo} was low, the amount of twist resulting from a speed change would not be large, and the static stability would not be seriously reduced; similarly if the wing centre of pressure and the flexural axis coincided, normal acceleration would produce no twist and the wing flexibility in torsion would not affect the manœuvrability.

He could see no reason why compressibility effects should alter these conclusions; if, as the sonic speed was approached the wing centre of pressure wandered there was little the designer could do beyond providing high stiffness, and the wing with low C_{mo} would at least start with an advantage. It was of course true that flexural axes were in general tending to move aft, and so to lower the divergence speed; this was an added reason for high stiffness. When they came to supersonic speeds, they were faced with a rather odd position: the tendency would probably be for aircraft to become very stable, owing to the backward movement of the aerodynamic centre, and it might be that they would wish that the destabilising influence of aeroelasticity were more marked! But it seemed likely that at supersonic speeds they would have symmetrical sections with the flexural axis at mid-chord, so that the effects of wing flexibility on longitudinal stability would be nil. Until they knew more of supersonic planforms and of their aerodynamic properties, such speculations, while of value in guiding thought on the subject, must be regarded as of academic interest only.

THE ROYAL AERONAUTICAL SOCIETY

POWER UNITS FOR FUTURE AIRCRAFT

by

AIR COMMODORE F. R. BANKS, C.B., O.B.E., R.A.F.V.R.

Air Commodore Banks joined The Ethyl Export Corporation in 1930 and was directly responsible for the technical development of leaded fuels used in air and land speed records for many years. He served at the Ministry of Aircraft Production during the war and was latterly Director of Engine Development. He has now rejoined The Associated Ethyl Company Ltd. as Technical Manager and Chief Engineer.

This Paper was originally written at the request of L'Association Francaise des Ingenieurs et Techniciens de L'Aeronautique (AFITA) and was presented at a meeting in Paris on 30th April, 1946. The Service Technique of the French Air Force were also interested.

The Paper reproduced here represents the entire lecture, except for those parts which were concerned purely with the French picture.

THE FUTURE OF THE PISTON ENGINE.

The piston engine has served us well for some forty years, but its well-earned place is about to be taken by the gas turbine.

The transition time will be about fifteen years; which may be divided, roughly, into 5-year periods.

In the next five years a number of prototype aircraft will be built with gas turbines. Turbine-jet and turbine-propeller units will be used, according to the type of aircraft and the operating requirements.

In from five years to ten years, successful gas turbine aircraft will be in production and the conventional aircraft with the piston engine will be in its decline.

In the period of the tenth to the fifteenth year, gas turbines will predominate for all important aircraft uses.

I see no reason why the gas turbine cannot meet every requirement at present filled by the piston engine, even to powers as low as 500 b.h.p. The turbine is completely superior to the piston engine for the very high powers.

There is, however, one requirement which the turbine cannot yet meet and this concerns fuel economy. For moderate cruising speeds around 300 m.p.h. and at altitudes of 25,000 ft. and below, the piston engine must still be considered where long range is wanted. But this appears to be the only case for retaining it. Aircraft of extreme range, say 10,000 miles, or having a radius of action of 3,000-5,000 miles, are necessary for certain military purposes; and I cannot see the gas turbine filling this need for some time to come.

Even the piston engine, for a range of 10,000 miles, must be developed to give considerably better fuel economy than it does now. I am thinking of the present cruising fuel consumption which can be obtained with the best piston engines, of 0.45 lb. per b.h.p./hr. and, in some cases, as low as 0.41 lb. per b.h.p./hr. To obtain a reasonable payload and to avoid oversize aircraft, which would have to be built mainly to accommodate the fuel, a cruising fuel consumption of not greater than 0.32 lb. per b.h.p./hr. should be the target.

To achieve a fuel economy of this order, some form of "compounding" is necessary, to give a higher expansion ratio to the working cycle of the engine. The obvious method

of attack is to utilise more fully the energy of the exhaust gases. We can, by using ejector exhaust systems, obtain measurable propulsive effort and so help to reduce the effects of drag; but this would not, in my opinion, give the overall improvement in fuel economy required for large aircraft of extreme range.

Compounding can be done by three methods: (a) an exhaust-driven turbine geared directly to the engine crankshaft by double reduction gearing and incorporating an "overrun" device; (b) an exhaust turbo-supercharger; and (c) by exhausting pairs of working cylinders, alternately, into a low pressure cylinder which is directly connected to the main crankshaft or to a separate crankshaft geared to the main shaft. A scheme similar to the last has already been suggested by Prescott at Wright Field, U.S.A.

Of the three methods, I favour (a). But if relatively high altitude operation is a requirement, then there is little alternative to (b), or a combination of (a) and (b).

When considering methods (a) and (c), the former should be considerably lighter than the latter, although engine weight is not a primary factor for extreme range and the main considerations must be reliability and economy.

There should not be any particular difficulty in designing and developing a reliable turbine and gearing for scheme (a), since there is now considerable experience of turbines of this size.

In the case of (c), the mechanical, or frictional, losses would be higher than those of a turbine arrangement.

I have spoken of compounding when applied to the present-day aviation engine, as a practical way of improving the fuel economy of existing engines, because a piston engine of completely new design would take some five years to develop and might be too late, in view of the advances which will be made by the gas turbine over the same period.

Had the gas turbine not become practical, I could visualise two further lines of development to achieve high fuel economy.

The first is an engine having very large cylinders of, say, 8 in. (203 m.m.) bore and of such a number as to give large total capacity. Large cylinders should have good thermal efficiency, due to the low surface-volume ratio of the combustion chamber. A fairly long stroke would, in my opinion, be desirable in order to attain the optimum speed of 3,250 f.p.m. with relatively low rotational speeds; this improves the mechanical efficiency and helps further to reduce the fuel consumption.

Compounding, in one form or another, would be necessary together with a high compression ratio and oil cooled pistons, etc. It would require a fuel of high anti-knock value and low sensitivity, since large cylinders would be particularly sensitive to detonation and pre-ignition. Water-alcohol injection may be used for take-off.

The second engine type for consideration is the two-stroke; which has been discussed, built, tested and abandoned with monotonous regularity during the past thirty or forty years.

There is a case for the two-stroke engine, but only if compounding is employed to make full use of the exhaust gas energy. By this means, a high degree of economy can be achieved and a cruising fuel consumption of, say, 0.36 lb. per b.h.p./hr. is possible with a gasoline two-stroke-turbine combination and about 0.32 lb. per b.h.p./hr. for a similar combination arranged for compression ignition heavy oil operation.

There is also the free piston engine, of which the Pescara is the classic example. But, I consider the free piston engine's main value is for industrial and marine purposes. It has many problems which must be solved before it can be suitable as an aviation engine. Its control particularly, and the cooling of the pistons, and so forth, are problems that will take time to overcome satisfactorily.

Problems do not deter the aviation engineer, otherwise we would not have progressed. But it is the matter of timing which, in a rapidly progressing art and science such as aviation, is so important. To develop a gas generator such as the Pescara and combine it with a turbine is a long job which may not be completed before a gas turbine proper of high economy arrives.

Returning to the question of developing existing engines to give economical operation, individual cylinder injection (of gasoline, *i.e.*, petrol) either directly into each cylinder or into the inlet valve port, close to the inlet valve, is recommended.

Considerable trouble has been experienced with the normal carburation systems and also with single point injection at the eye of the blower, due to mal-distribution of the air-fuel mixture to the cylinders. This trouble is more prevalent with in-line than with radial engines and, particularly, when operating for maximum economy where the engine speed is much reduced and the mixture temperature is low.

The heavy or high boiling fractions of the fuel go to some cylinders and the lighter or low boiling fractions into others. The heavy ends take with them a large proportion of the tetraethyl lead and little of the halogen bearer, ethylene dibromide. The lighter or low boiling fractions are robbed of the lead, but are accompanied by a high proportion of the ethylene dibromide.

The result of this physical inequality is to give a high rate of deposition of combustion products in those cylinders receiving the large proportion of heavy ends and tetraethyl lead, usually causing sparking plug fouling by lead compounds which, in turn, leads to rough running and vibration.

In consequence, also, there is a variation in the anti-knock value of the fuel, according to its fractions and lead concentration. Since the performance of an engine is controlled by detonation—all other things being equal—it follows that mal-distribution will limit the power output to that allowed by the particular cylinders receiving fuel having a lower detonation value than the fuel in the tank or as it was originally composed.

Therefore, in order to obtain the best results, both as regards reliable operation and freedom from frequent ground inspection between servicing periods and to make the best possible use of the anti-knock qualities of the fuel, direct injection to the individual cylinders is essential in my opinion.

Fuels for piston engines.

There is little I can add to that already known about fuels. But since the emphasis in future will be on fuel economy, this will mean a weak mixture rating higher than the present standard aviation fuel of 100-130 grade.

Fuels of the following ratings could be made available, *at a price*, if desired: 115-145, 125-165, 135-170 and perhaps 150-200. But it must be appreciated that such fuels, particularly in the very high gradings, would restrict production and would be costly to produce. For special purposes, on a particular route such as the Atlantic crossing, the extra cost of the fuel might be outweighed by the benefits of increased payload, always supposing that the fullest use is being made of the fuel qualities.

With regard to the maximum concentration of tetraethyl lead to be used, it is not expected that this will be above 5.5 ccs. per Imperial gallon (1.2 ccs. per litre). But I do not expect the concentration to be lower than this figure for the highly graded fuels, because of the beneficial influence of lead upon the weak mixture rating and because the use of lead materially assists in the reduction of refinery costs and manufacture of the fuel.

"Safety" or high flash-point fuels.

The development of direct injection opens the way to the use of the so-called "safety fuels." These are high flash-point fuels of 100°F. (38-40°C.) having much the same anti-knock properties as present aviation fuel.

The question of how safe these fuels are is still undetermined. There may be a greater danger of explosive fuel-air mixtures in the aircraft tank with them than with the more volatile fuels used at present. This would have serious disadvantages, unless the aircraft fuel tanks were " blanketed " with some inert gas.

To sum up, the use of " safety " fuels will depend upon the results obtained; first in the development phases of the engine and aircraft as a whole and after this, their real value, negative or otherwise, will only be determined after many thousands of aircraft flying hours. It is an unfortunate fact that the alleged security of " safety " fuel can only be proved after a crash! And a crash of the usual nature, *i.e.*, hitting high ground in bad visibility, usually means that most of the passengers and crew are killed by impact. In actual fact, there have been very few fires or explosions *in the air* in the whole history of civil aviation.

THE GAS TURBINE.

We now come to the gas turbine. This has been developed to a practical state during the war years, in its " jet " form.

A turbine driven by the exhaust gases of a piston engine and connected to a blower in order to maintain the sea level power of the engine at altitude, was originally conceived by Rateau. This idea was carried a step further, to the practical flight stage, by the R.A.E., Farnborough, and then abandoned. But Wright Field, with General Electric, in the U.S.A. brought it to a high state of development and it is now fitted to a large number of American aircraft.

The advent of the Whittle turbine-jet unit has carried the turbine forward to a position where, after relatively few years of intensive development, it has virtually taken the place of the piston engine in pursuit aircraft and now threatens the piston engine in all other fields of aviation, with the possible, but only temporary, exception of the requirement for fuel economy.

The Whittle is an internal combustion engine and it drives its own blower which discharges to combustion chambers located, integrally, between blower and turbine.

The gas turbine, like the piston engine, is basically an air pump but, whereas the piston engine has a timed or controlled respiratory cycle, with its inherent mechanical limitations, the gas turbine does not suffer these restrictions. It works on a continuous (untimed) combustion cycle and since its mechanical system is rotational it can deal with vastly more air than the piston engine.

There are certain disadvantages which prevent it from taking the place of the piston engine completely. The operating temperatures of the latter are much higher than those at present possible with the gas turbine. The maximum working temperature within the piston engine, according to compression ratio and mixture strength, etc., varies from about 2,500°C. to 3,000°C. absolute. Further, the working fluid of air and hydro-carbon fuel is not diluted with the excess air, which is necessary in the case of the gas turbine where at present a temperature of 1,100°C. absolute at the turbine entry cannot be exceeded, because of material limitations.

It will be appreciated, therefore, that these temperature limitations prevent the gas turbine from attaining the thermal efficiency of the piston engine. But this will not necessarily be so, in future. In any case, the gas turbine has other advantages.

The turbine-jet engine is considerably lighter than the piston engine of equivalent power, both in regard to its " dry " and " installed " weights. It does not need any coolant because it is internally cooled by its own excess air. Present gas turbines run on ball and roller bearings so that a large quantity of oil is unnecessary and oil cooling is not required.

The specific weight of a simple turbine-jet engine is about 0.3 lb. per lb. of static thrust. Propeller-turbines will have somewhat higher weight, between 0.75 lb. and 0.90 lb. per equivalent shaft horsepower.

Future turbines, both of the " jet " and also the propeller or power types, will tend to become more complex when designed for high fuel economy and a specific weight of 1 lb. to 1.5 lb. per equivalent shaft horse-power is to be expected in the latter (propeller) case. These figures are related to maximum power at sea level.

Gas turbines may be divided into two general categories: (1) turbine-jets, and (2) propeller or power turbines.

In the case of (1), there is the Whittle type which consists of a single stage, double-sided, centrifugal compressor and single stage turbine. One variant of this is the de Havilland Goblin, which has a single stage and single-sided compressor.

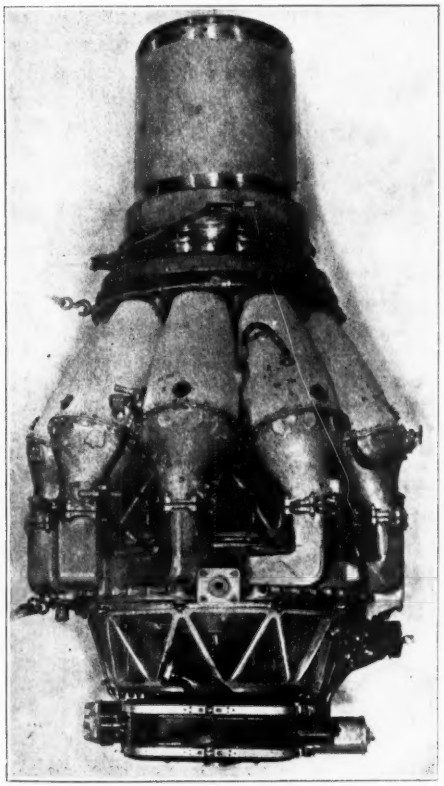


Fig. 1.
Rolls-Royce Derwent V.

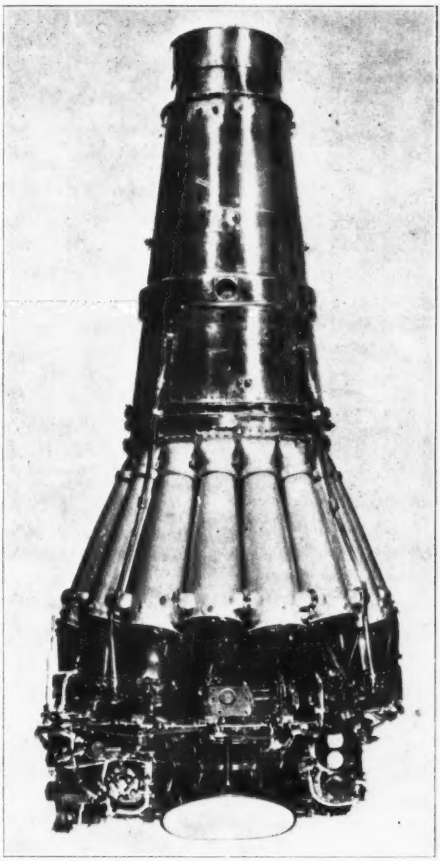


Fig. 2.
de Havilland Goblin.

Figure 1 shows the Whittle-type Rolls-Royce Derwent V turbine-jet engine, which has a maximum thrust (static) of 4,200 lb. for a weight of approximately 1,250 lb. Fig. 2 is the de Havilland Goblin, now giving 3,750 lb. thrust (static) for a weight of 1,560 lb. Figs. 3 and 4 give the performance curves of these two engines, respectively.

General arrangements of representative propeller or power turbines are given in Figs. 5, 6 and 7.

Figure 5 is the Armstrong-Siddeley Python, which has an axial flow compressor directly coupled to a two-stage turbine. The

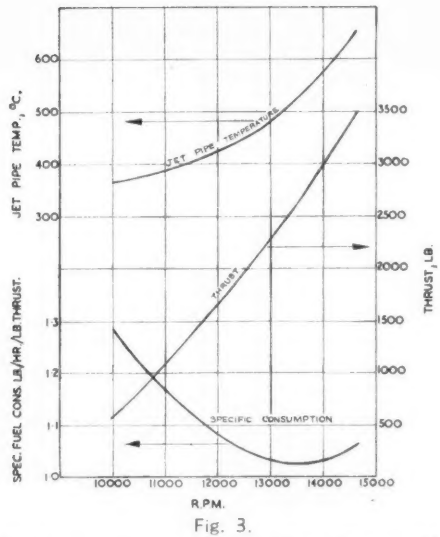


Fig. 3.
Test bed performance of Rolls-Royce Derwent V turbine-jet engine.

propeller is driven through a double-reduction gear, at the front end. The pressure ratio of the compressor is 5-1, and the propeller reduction gear ratio is 8.13-1. The power is 3,600 h.p. at the propeller shaft, plus 1,150 lb. thrust (static) at the jet pipe. The operating speed is 8,000 r.p.m. The weight is 3,140 lb. without propeller.

Figure 6 shows the Bristol Theseus. This has an axial flow and a centrifugal compressor, in series. The centrifugal compressor is located between the axial compressor and the combustion chambers. The theoretical advantages claimed for this arrangement are that the centrifugal compressor assists the axial compressor over its relatively narrow operating range, particularly during the starting of the engine, thus avoiding instability and promoting flexibility of operation. These advantages are, however, only theoretical and more experience must be gained before this arrangement can be proved.

There is one important advantage in the location of the centrifugal compressor. The air from the discharge of the axial compressor must be turned sharply when separate combustion chambers are used; if a centrifugal compressor is considered necessary for other reasons it is a convenient substitute for sharp pipe bends and also provides an increase in total pressure ratio.

xial and centrifugal compressors of the Theseus are driven by a two-stage turbine. There is another turbine wheel fitted to a shaft which passes through the hollow shaft of the two-stage turbine and compressors. This turbine wheel independently drives the propeller through double reduction gearing.

In the particular case of the Theseus, by the mechanical separation of the propeller turbine from the compressor it is hoped that some flexibility may be obtained such as, for example, overspeeding the propeller for take-off. But this may involve an increase in temperature at the inlet to the high pressure turbine. This flexibility, if realised in practice, will also enable the pilot to use a lower propeller speed when landing and taxying.

The separation of the propeller from the compressors is also a help in reducing the

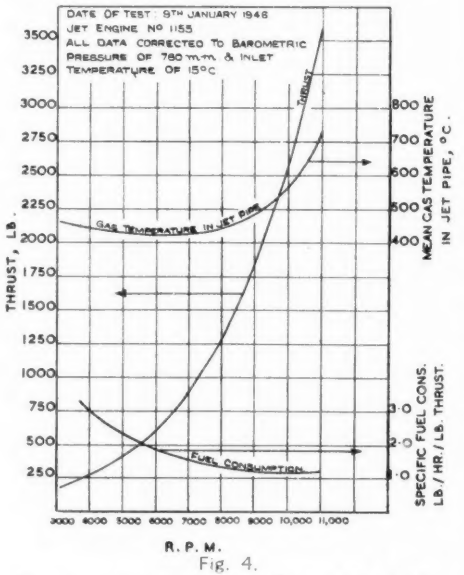

DATE OF TEST 9TH JANUARY 1946
JET ENGINE No 1155
ALL DATA CORRECTED TO BAROMETRIC PRESSURE OF 760 m.m. & INLET TEMPERATURE OF 15°C
THRUST
GAS TEMPERATURE IN JET PIPE
FUEL CONSUMPTION
THRUST, LB.
MEAN GAS TEMPERATURE IN JET PIPE, °C.
SPECIFIC FUEL CONS. LB./HR./LB. THRUST.
R.P.M.

Fig. 4.
Performance of de Havilland Goblin turbine-jet.

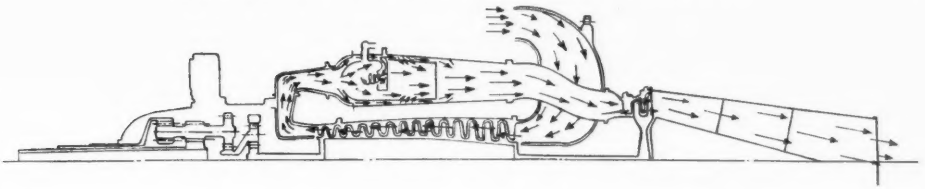


Fig. 5.
Armstrong Siddeley Python.

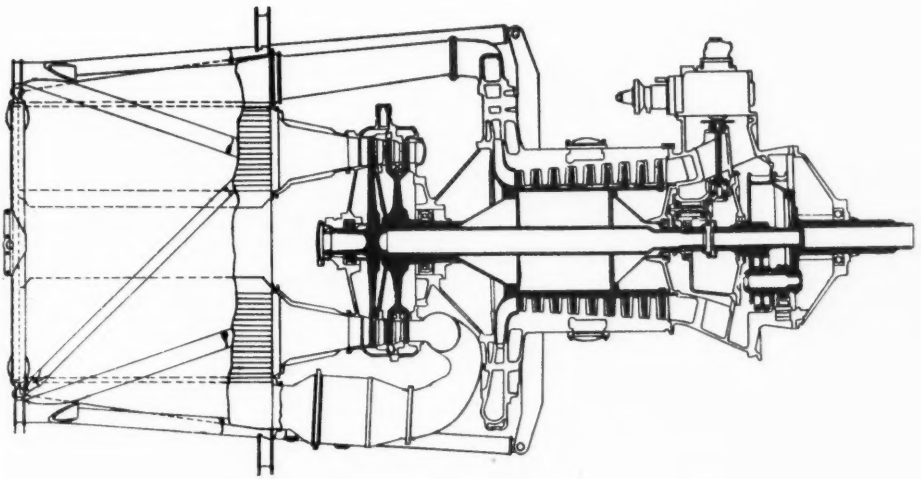


Fig. 6.
Bristol Theseus I.

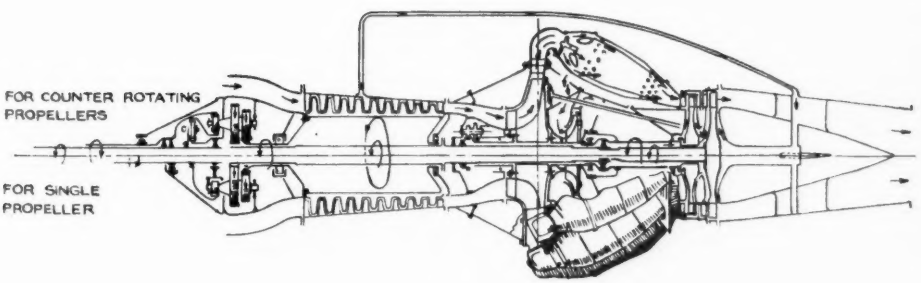


Fig. 7.
Rolls-Royce Clyde. Diagram of Airflow.

Note—In this diagram the combustion chambers being shown in the same plane as the engine axis, the angle of bend of gas flow is unavoidably exaggerated. The eleven combustion chambers lie on the skew as indicated above.

power required for starting, because no power is absorbed in turning the propeller.

A torque dynamometer is incorporated in the propeller reduction gear.

The Theseus engine has been designed to incorporate a heat exchanger for improved fuel economy. It is expected that a reduction in the order of 10 per cent. in specific fuel consumption will be achieved at small expense in power but appreciable increase in weight. The compressors (axial and centrifugal) have an overall pressure ratio of 4.35-1. The shaft horsepower, at the propeller, is 1,950 h.p. with 500 lb. thrust (static) in the jet pipe. The engine speed is 9,000 r.p.m., and its weight is 2,500 lb., without propeller.

Figure 7 shows the general arrangement of the Rolls-Royce Clyde. In this case, the first turbine wheel drives the centrifugal compressor and the second turbine drives the axial compressor and also the propeller, through double-reduction gearing. The arrangement shown is for contra-rotating propellers.

The power is 3,000 h.p. at the propeller shaft, with 1,200 lb. thrust (static) in the jet pipe. The engine speed is 6,000 r.p.m. The dry weight, without propeller, is about 2,500 lb.

I have described what are, at present, the typical forms of the gas turbine. I now propose to deal with the turbine engine in more detail.

The gas turbine works on the "constant pressure" thermodynamic cycle. This can be demonstrated more clearly by a temperature-entropy diagram which is shown in Fig. 8.

Referring to the diagram; the full line (0, 1′, 2′, 3′, 4′, 5′) represents the theoretical cycle, assuming no losses. Atmospheric air, in a condition represented by point 0, is raised in temperature and pressure to conditions corresponding to point 1′ by the effect of "ram." Point 2′ is reached after the air has passed through the compressor. From 2′ to 3′ heat is added at constant pressure, in

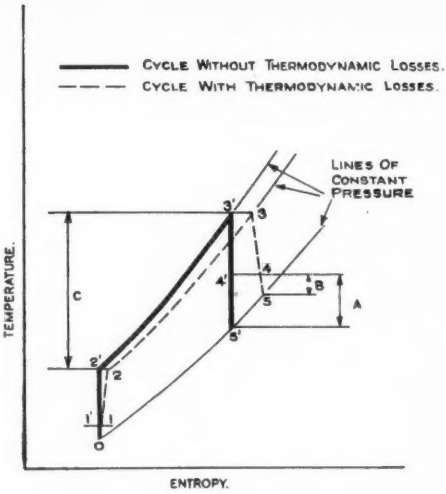


Fig. 8.
Diagram of thermodynamic cycle.

an amount proportional to the temperature difference, C. The gas is then expanded from 3′ to 5′ where the pressure is, again, atmospheric.

The energy available in the expansion is proportional to the temperature drop from 3′ to 5′. A proportion of this energy 3′ to 4′, equal to 1′ to 2′, is required to drive the compressor. And the remaining energy, 4′ to 5′, proportional to *A*, may be used, in jet form, to propel the aircraft, or some may be converted into shaft power by means of a turbine.

The actual cycle, with losses taken into account, is shown by the dotted line (0, 1, 2, 3, 4, 5). With the same input of work into the compressor, the resultant pressure at 2 is lower than 2′. Because of this lower pressure ratio and, also, due to the losses during expansion, the available energy in expansion, 3 to 5, is much smaller.

The work required to drive the compressor is, however, unaffected and consequently the useful net energy available, 4 to 5, proportional to *B*, is much reduced. The thermal input for both the cycles shown is

proportional to C. Therefore, the thermodynamic efficiency of the cycle, with no losses, is $\frac{A}{C}$ and, with losses, is $\frac{B}{C}$.

The diagram (Fig. 8) shows the great importance of achieving low losses in the compressor, turbine and combustion chamber to obtain good efficiency in the gas turbine as a whole.

The temperature-entropy diagram may be used to illustrate the effect of increasing compression ratio and gas temperature at the inlet to the turbine. Figs. 9 to 16 also show some results of calculations on these effects.

Output per unit rate of air flow and specific fuel consumption are shown on these figures, for simple jet and also propeller turbines. Two sets of figures are shown for each type of engine, one set for sea level static conditions, and the other for flight at 800 km./hr. (500 m.p.h.) at the tropopause, about 11 km. (36,000 ft.) altitude. In the case of the propeller turbine it will be seen that, as the turbine inlet temperature is increased, the specific consumption decreases and the specific output rises. As the pressure ratio is increased, at a given turbine inlet temperature, the specific consumption passes through a minimum and the specific output through a maximum. But it will be noted that these two optimum points do not occur at the same pressure ratio. This is particularly marked in the curves for 800°C. absolute and 1,000°C. absolute and is due to the increasing effect of losses in compression and expansion which offset any thermodynamic gain.

The effect of pressure ratio and temperature on the performance of simple jet engines, shown in Figs. 9 to 12, is complicated by the additional factor of propulsive efficiency. The specific thrust varies in approximately the same way as the specific power output of propeller turbines. The specific consumption, however, generally rises as the temperature increases. This is the result of decreases in propulsive efficiency which occur when the jet velocity is increased.

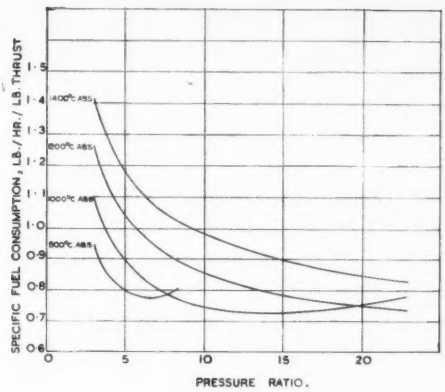


Fig. 9.
Specific fuel consumption of turbine-jet engines. Static sea level conditions.

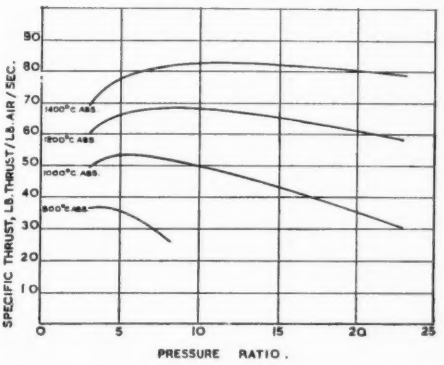


Fig. 10.
Thrust per lb. of air for turbine-jet at static sea level conditions.

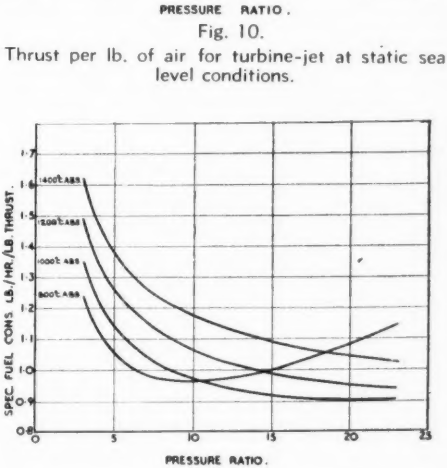


Fig. 11.
Specific fuel consumption of turbine-jet at 500 m.p.h. I.C.A.N. tropopause.

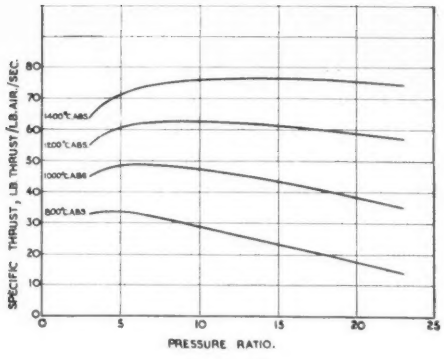


Fig. 12.

Specific thrust per lb. of air for turbine-jet at 500 m.p.h. I.C.A.N. tropopause.

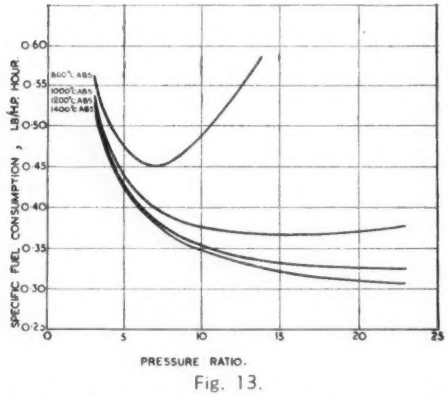


Fig. 13.

Specific fuel consumption of turbine-propeller combination at 500 m.p.h. I.C.A.N. tropopause.

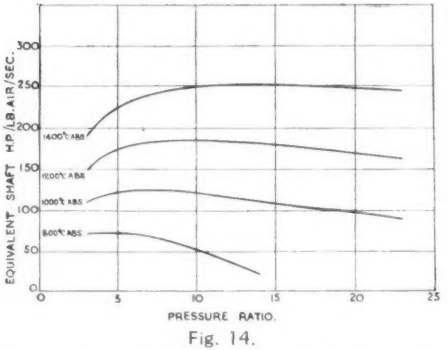


Fig. 14.

Power output per lb. of air for turbine-propeller combination at 500 m.p.h. I.C.A.N. tropopause.

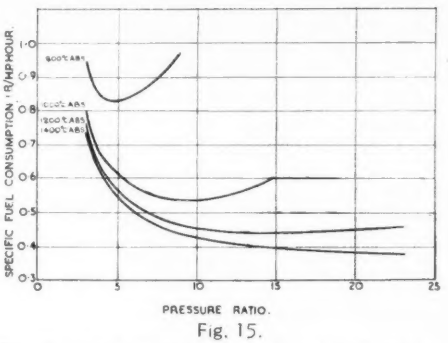


Fig. 15.

Specific fuel consumption of turbine-propeller combination. Static sea level conditions.

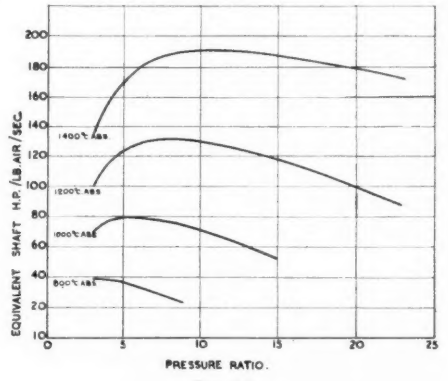


Fig. 16.

Power output per lb. of air for turbine-propeller combination. Static sea level conditions.

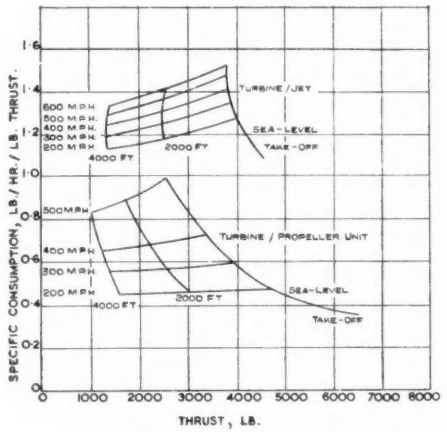


Fig. 17.

Fuel consumption-thrust curves for simple jet engine and turbine-propeller combination.

At any given temperature there is an optimum pressure ratio which is shown most definitely on the curves for 800°C. absolute. But, for temperatures now used, this occurs at a large value of the pressure ratio which is above that giving the maximum thrust.

Up to the present time we have been using pressure ratios between 4-1 and 6-1 for simple jet engines and, although higher pressure ratios are desirable for thermodynamic reasons, the increased weight and greater complication involved may largely counterbalance any improvement in thermodynamic efficiency.

The curves which I have just discussed, in Figs. 9 to 16, have been constructed for the following conditions:—

(a) Compressor polytropic efficiency = 87%
(b) Compressor turbine overall efficiency = 87%
(c) Jet pipe expansion efficiency = 95%
(d) Power turbine efficiency = 88%
(e) Assumed propeller efficiency = 75% at sea level; = 74% at tropopause
(f) Equivalent jet brake horsepower at sea level static = $\frac{\text{jet thrust}}{2.5}$
(g) Combustion chamber pressure loss in "pounds per square inch" = 2 × relative atmospheric pressure.

Following these considerations, Fig. 17 gives characteristic curves of fuel consumption against thrust at different speeds from sea level to (approximately) the tropopause; for (a) a simple jet engine of the Whittle type and (b) a propeller turbine having two compressors, in series.

It will, perhaps, be of interest if some idea is given of the working conditions inside a simple jet engine of the Whittle type, when running at normal maximum speed. A section of the Rolls-Royce Derwent engine (Fig. 18), represents a typical Whittle type. Other figures show a section, through one combustion chamber, of this type of engine, giving the particular conditions existing throughout its system when it is running.

Figure 19 shows the temperature conditions, from the compressor inlet to the jet pipe. Fig. 20 gives the pressure distribution and Fig. 21 the Mach number at the various points. In Fig. 22, the principal stresses existing in the compressor rotor, the turbine disc and turbine blade roots are given.

The air consumption of the gas turbine-jet engine is enormous and the accepted figure for normal assessment is 1 lb. of air per second for every 50 lb. of thrust. This means that the Derwent V engine, giving 4,200 lb. thrust, consumes about two tons of air per minute, or over 130 tons per hour! With compressors of higher efficiency and when turbines can work at higher gas temperatures, we should be able to obtain 60 lb. of thrust per lb of air per second.

As mentioned earlier in this section, the gas turbine cannot operate at the relatively high temperatures of the normal piston engine. This is due to the present limitations of material and cooling. Except in the primary zone, the overall air-fuel ratio is very high and in the region of 60-70-1, compared with that for the piston engine which approaches the value for chemically correct combustion of about 15-1.

In the simple jet engine the turbine wheel is employed only to drive the compressor for pumping the large mass of air, which, after the addition of heat in the form of fuel, is ejected through the propelling nozzle at high speed—approximately 1,750 feet per second. The reaction of the hot gases provides the propulsive effort.

The propeller or power turbine, already discussed, is designed so that more of the power, or gas energy, is absorbed by the turbine wheel, or wheels, than is the case

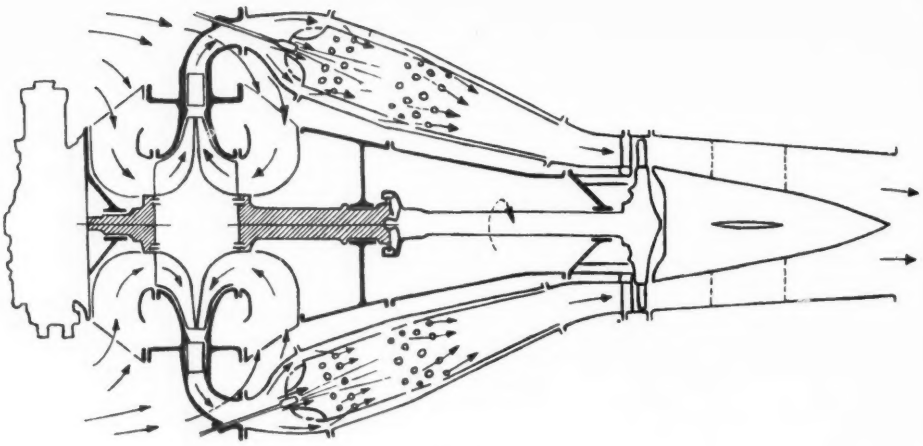


Fig. 18.
Rolls-Royce Whittle-type Derwent. Diagram of Airflow.

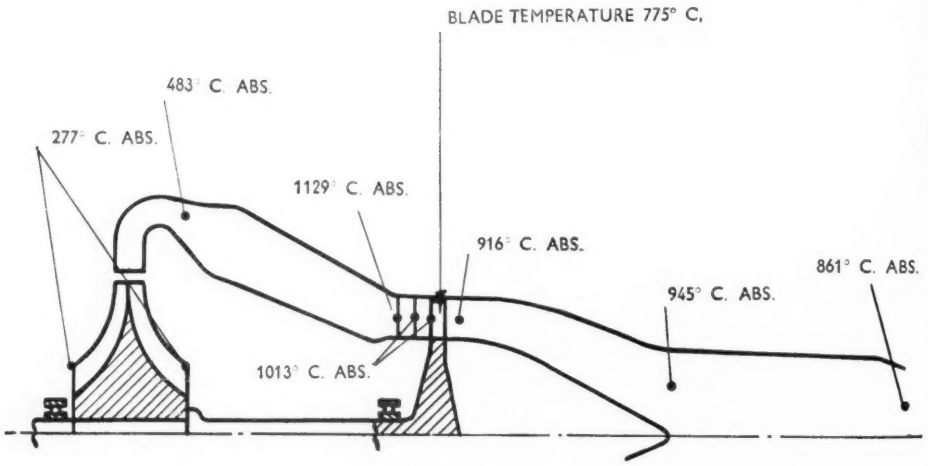


Fig. 19.
Temperature distribution in a Whittle-type jet engine.

with the jet turbine. This provides power, both for the compressor and the propeller. But less jet energy is then available and the gases leave the propelling nozzle at reduced velocity.

Propeller turbines can be designed to meet a fairly wide range of aircraft operating conditions and the power at the propeller shaft and in the form of jet energy can be proportioned accordingly; for instance, 90 per cent. shaft horsepower and 10 per cent. jet thrust or 75 per cent horsepower and 25 per cent. jet thrust, etc., etc.

There is also the turbine-jet engine with an augmentor or ducted fan. This takes the form of an axial type fan in an annular duct surrounding the main turbine. The fan can be connected to a ring of turbine blades which are located in the gas stream behind the turbine proper. Another method is to

drive the fan mechanically, direct or through gearing, and locate it at or near the air entry of the compressor. The fan duct surrounds the engine and is carried rearwards where it can discharge as an annular jet or into the jet pipe proper, to mix with gases there. One of the advantages of locating the fan at the compressor entry is that some degree of ram or supercharge is given to the air entering the compressor. (See also Fig. 23).

The advantages claimed for the ducted fan, or augmentor, are that it provides greater thrust than the simple jet during take-off and the climb of the aircraft. But the thrust advantage over the ordinary jet decreases with forward speed, dropping to zero at about 550 m.p.h. The ducted fan in its simplest form has a lower weight than the propeller and less mechanical complication. But it is probable that, in order to obtain the best from the ducted fan, it will tend to become more complicated and may have to have blades with automatic pitch variation. In addition, variable area propelling nozzles will have to be provided.

Further, the ducted fan will not have the complete flexibility of the propeller, but this disadvantage could, to some extent, be over-

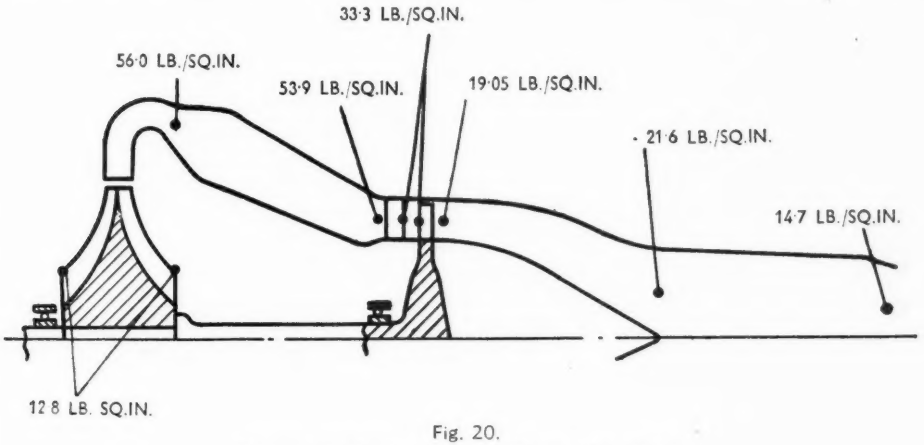


Fig. 20.
Pressure distribution in a Whittle-type jet engine.

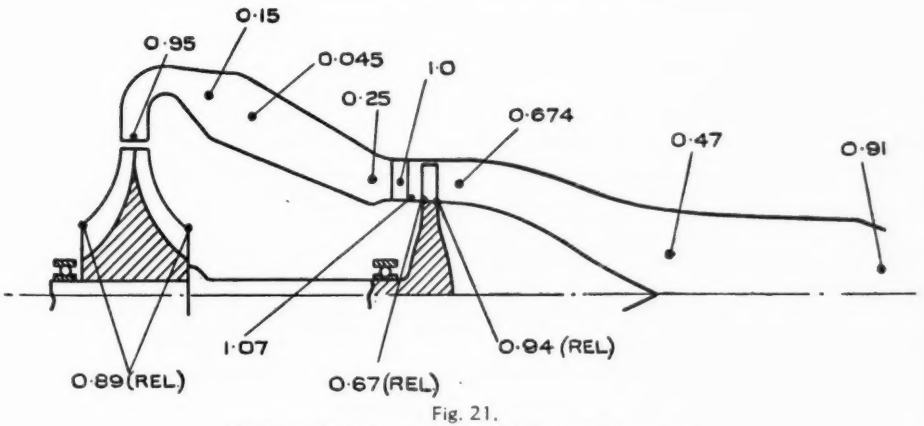


Fig. 21.
Variation of Mach number in Whittle-type jet engine.

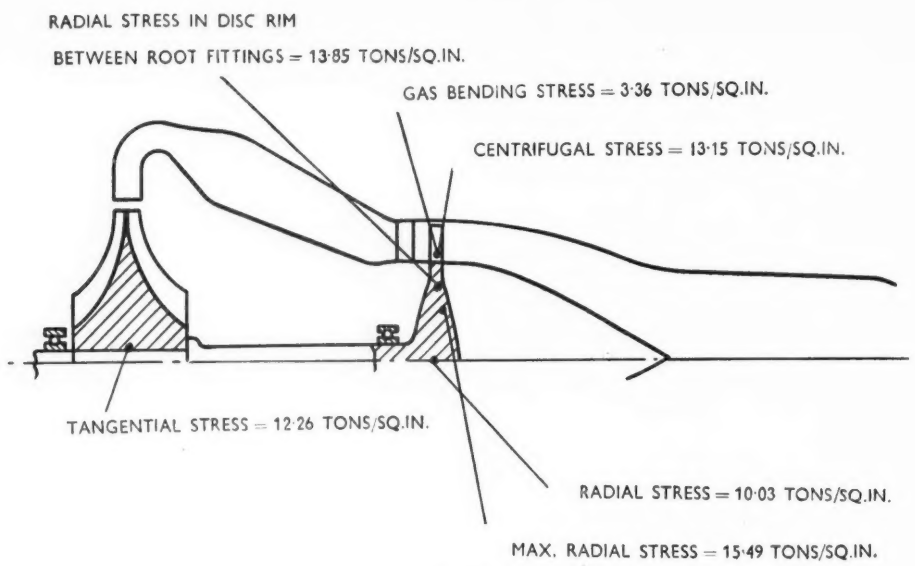

RADIAL STRESS IN DISC RIM
BETWEEN ROOT FITTINGS = 13·85 TONS/SQ.IN.
GAS BENDING STRESS = 3·36 TONS/SQ.IN.
CENTRIFUGAL STRESS = 13·15 TONS/SQ.IN.
TANGENTIAL STRESS = 12·26 TONS/SQ.IN.
RADIAL STRESS = 10·03 TONS/SQ.IN.
MAX. RADIAL STRESS = 15·49 TONS/SQ.IN.

Fig. 22.
Principal stresses in a Whittle-type jet engine.

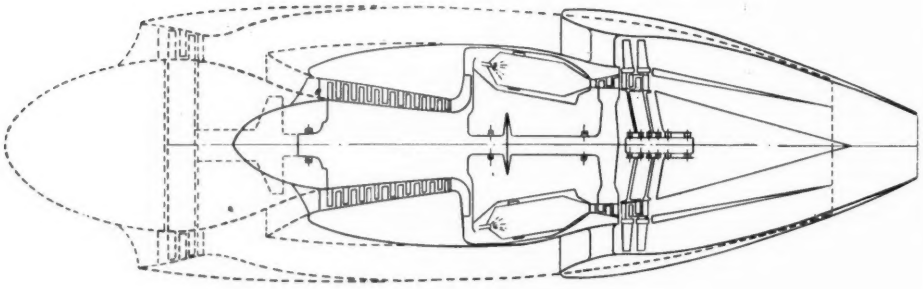


Fig. 23.
Free turbine augmentor (full line) or positive-drive ducted fan (broken line).

come by burning fuel in the duct behind the fan to increase the thrust, similar to burning fuel in the jet pipe—which has already been tried with encouraging results. However, it is uneconomical to burn fuel in this manner. The control problems of the ducted fan engine may be formidable and the aircraft designer must first be reasonably satisfied that it gives him the means of producing a cleaner aircraft. It should be less noisy than a propeller.

In the case of the simple jet, the ducted fan and the propeller turbine, their propulsive efficiency is highest when there is least difference in speed between their respective "discharges" and the aircraft in which they are fitted. This assumes that efficiency is assessed on a change of kinetic energy basis, where input per lb. of air flow is equal to the jet, or discharge, kinetic energy, less the kinetic energy of forward speed

$$\left(\frac{V_j^2}{2g} - \frac{V^2}{2g}\right)$$

where V_j is jet velocity and V is aircraft

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velocity both expressed in feet per second.

It will, therefore, be appreciated that the aircraft fitted with the simple jet engine must fly at high speed for the best possible propulsive efficiency and to obtain reasonable operating economy. The present (best) value for specific fuel consumption, on the basis of static or test bed thrust, is about one pound of fuel per pound of thrust per hour (1 lb. per lb./hr.). To be able to fly fast and relatively economically, the aircraft must also fly at great altitude, where the density of the air is low and drag is consequently reduced. When it is possible to build an aircraft with reliable, safe and comfortable pressurisation and when all radio and radar aids are available enabling such an aircraft, flying at speeds in the region of 550 or 600 m.p.h., to navigate and be handled safely over and on to aerodromes, then, I submit, the simple jet will largely take the place of the other turbine variants.

So far, only simple jet engines have been mentioned of the true Whittle type, having centrifugal compressors and separate combustion chambers. There are naturally, variants which employ axial compressors and have either a number of separate combustion chambers or a single annular combustion chamber.

It is still necessary to improve the fuel consumption of the simple jet engine. This can be done by (a) higher efficiency compressors, (b) combustion chambers having a still lower pressure drop and (c) more efficient turbines. The latter (c), so far as the simple jet is concerned, does not have an important influence upon fuel consumption within one or two per cent. of the optimum efficiency. But, when related to propeller turbines, the efficiency is of very great importance; a difference of two or three per cent. in turbine efficiency, in this case, will affect the fuel consumption by as much as eight or nine per cent.

In the case of (a), the axial compressor may supersede the centrifugal type, because the axial offers the advantages of higher pressure ratios and higher adiabatic efficiency. But it is not yet absolutely clear that this will be the case universally. The centrifugal compressor of the two-stage type should be capable of development to give a pressure ratio of 6-1 (sea level) with an adiabatic efficiency of 80 per cent. An axial compressor for the same pressure ratio would, today, give an adiabatic efficiency of, say, 85 per cent., but an efficiency of 88 per cent. is considered attainable, eventually.

Axial compressor turbines offer opportunities for small diameter for the same power, relative to the centrifugal types, particularly when the annular combustion chamber is used. It is now considered that the former will not weigh any more than the latter, which was not the case a year or so ago. Actually, there are in existence axial-type engines weighing no more than their centrifugal counterparts.

Some disadvantages of the axial type are that it is more costly to produce and it is probably more critical to accidental damage because of its many light blades. Also, it is critical to the condition of its blades which, if dirt builds up on their surfaces, generally causes a serious loss of efficiency and drop in performance.

It is doubtful, however, whether there is any advantage in exceeding a pressure ratio of about 6-1 for the centrifugal compressor of the simple jet engine, because the efficiency of compression tends to fall beyond this value. However, in the case of the propeller turbine high pressure ratios are very necessary to achieve high operating economy and the axial compressor is the only way to obtain high pressure ratios, together with relatively high adiabatic efficiency. Compounding, *i.e.*, using compressors in series, will probably be necessary at the high pressure ratios in order to obtain the maximum degree of flexibility and to avoid stalling at low rotational speeds. It is necessary to obtain as high an adiabatic efficiency as possible, otherwise the power required to

drive the compressor will increase disproportionately.

In small, short-range aircraft of the high-speed pursuit type, the length of the axial may be almost more embarrassing than the somewhat larger diameter, but shorter, centrifugal compressor. Because if the engine is placed in the fuselage its diameter may matter little but its length may prejudice seriously the arrangement of guns and fuel tanks. The air intakes of the engine which are of paramount importance, must not suffer loss in efficiency, even for the gun installation. It is not now practicable to put the fuel and guns in the thin wings of the high-speed jet fighter.

Thus, it may be necessary in some cases to mount engines on the wings in order to provide adequate power and to leave space in the fuselage for fuel and military equipment. In this position the air entry conditions for the engines will also be good.

On the question of separate (multi) versus annular combustion chambers; practically all British engines use the former type of combustion system. The advantages are that it is a relatively simple matter to test one unit (chamber) and know that it is representative of those eventually to be fitted to the engine. Also, it only takes a fraction of the total equivalent engine air to test one of a number of chambers.

The annular combustion chamber, however, requires the total air necessary for the engine and, therefore, is a much more expensive test consideration. It is unsatisfactory to test sections of an annular chamber since it is not possible to reproduce the correct flow conditions. Because of these difficulties and a previous lack of full-scale air supply, the annular combustion chamber has received less development than the multiple or separate type. The advantages of the annular system are its small relative diameter and the fact that it should have a lower pressure drop than the multiple system.

If, and when, a successful and durable annular combustion chamber is developed full advantage can then be taken of the small diameter of the axial type compressor and it should be possible to build a jet engine giving about 3,300 lb. thrust for a diameter of 24 in.

However, the problems of size and weight bring in other factors. I have discussed the efficiency of the gas turbine and the influence of compression ratio and temperature on its efficiency and power output. But, in order to design compact and light turbine engines it is necessary to obtain the largest output of work per stage of both the compressor and the turbine. It is also necessary to have the highest flow rates possible in order to obtain an engine of minimum total cross-sectional area. To achieve this, high flow velocities through the components are required, extending well into the compressibility region. In addition, high blade speeds are desirable but this obviously leads to high stresses. Consequently, the design, as in all mechanical engineering, must be a compromise which, in this case, is one between the stressing of the turbine and its blading and the maximum flow velocities or Mach numbers which can be obtained without any appreciable loss in efficiency.

When referring to stresses it is the British practice also to specify a value for the gyroscopic loading which must be allowed for in the design of the turbine, to meet the aircraft manoeuvring conditions; this figure is 3.5 radians per second.

The gas turbine is a " full-throttle " engine and normally runs at, or near, maximum conditions. It has therefore, the characteristics of such engines and the power falls off from sea level to altitude. The turbine is also critical to air inlet temperature and it is important to ensure that, when meeting a new aircraft requirement, allowance is made for the take-off and climb conditions in hot climates and also, under similar conditions, from high altitude aerodromes.

For instance, an aircraft flying from Europe to Australia could lose anything up to 20 per cent. of its thrust at take-off from

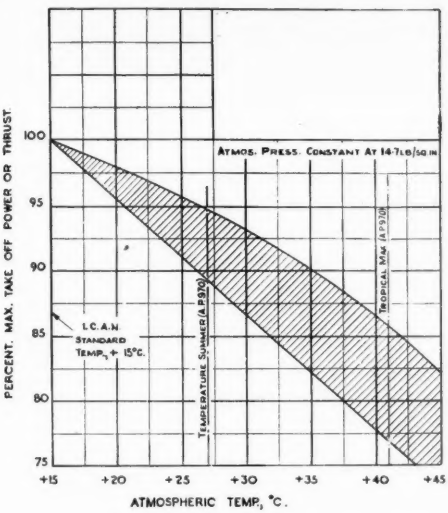

Fig. 24.

Effect of atmospheric temperature on S.L. static power of gas turbines.

NOTE.—The shaded area covers the engine constructors' calculated performance for the following types of gas turbines—simple J.P. and propeller-turbine and compound propeller-turbine engines. This method of presentation was chosen in view of design variations such as compression ratio and differing engine operating conditions.

the Far Eastern aerodrome en route. (See also Fig. 24).

The size or capacity of the engine must, therefore, be accurately assessed for all the known and possible aircraft operating conditions.

TURBINE FUELS.

In the future there will be a real problem in the supply of gas turbine fuels, both in regard to quality and quantity. The present fuel used is a kerosine which has a paraffinic base and a boiling range between 150°C. and 200°C. The freezing point is in the region of —40°C. But kerosine may not be available in sufficiently large quan y when gas turbines are in the majority, unless other demands for it can be reduced, or if it is produced by synthesis. This may eventually be necessary.

An aromatic kerosene is relatively easy to produce in quantity but aromatic fuels are not easy to burn without the formation of considerable carbon deposit, which can build up in the combustion system, reduce combustion efficiency and cause local over-heating of the flame tubes. It will be necessary to test such fuels and endeavour to overcome these troubles.

Gas oil is another promising fuel which should burn satisfactorily, but a combustion system must be developed to use it. A serious disadvantage of the normal gas oil is its high freezing point of 0°C. to about —10°C., according to its constitution and refining. This would probably necessitate heating the fuel in the aircraft tanks and pipe lines. But I believe the oil companies could, if pressed to do so, produce a gas oil with a freezing point of —40°C. and perhaps lower.

The most easily obtainable fuel in quantity and one which should be very satisfactory in regard to combustion and also freezing point (about —70°C. and lower), is ordinary low octane motor fuel (gasoline), preferably of paraffinic base and without tetraethyl lead. The disadvantage of gasoline or, rather, the advantage of the fuels of higher specific gravity, such as kerosine and gas oil, is that their calorific value per unit volume is relatively high and, assuming a given tank volume, these fuels will give about 8 to 10 per cent. improvement in volumetric fuel consumption, relative to gasoline. But perhaps when there has been full opportunity to test all the likely fuels low octane motor fuel will be quite strongly favoured with a vapour pressure adjusted to suit aviation requirements, *i.e.*, not more than seven pounds Reid.

It will be appreciated that there is much to do before a specification can be written for a satisfactory fuel—volatility, vapour pressure, combustion, freezing point and supply, and so on.

On the particular point of combustion, there has been considerable difficulty in maintaining stable combustion conditions at altitudes around 35,000-40,000 feet and the flame has extinguished itself at about these

altitudes (rich mixture blow-out); also, there has been failure to re-light after stopping the engine. The real cause, or causes, are obscure at the moment but, in my opinion, it is mainly due to ineffective distribution of the fuel in the available oxygen (air) and the difficulty of initiating combustion in over-rich zones of mixture and at extremely low barometric pressures (180 mm./hg.). I think personally, the vapour pressure and-or the volatility of the fuel will be found to have considerable influence on this phenomenon.

Already, there are some indications that gasoline (in this particular case aviation 100-130 grade), with its relatively high vapour pressure and volatility, compared with kerosine, has not shown the same inclination to give trouble in this way. Recent tests with aviation fuel have shown combustion to be maintained under the same conditions which caused the so-called rich mixture blow-out with kerosine.

It appears that a healthy electric spark will assist the combustion process at these unstable conditions but this should only be considered as a palliative and a proper cure must come from a thorough understanding and, then, treatment of the problem.

Since the low pressures at these altitudes obviously have a great influence on the combustion process, it is equally obvious that when higher pressure ratio compressors than those in use at present are in service, the (higher) pressures which will then pertain in the combustion system (at the same altitudes) will improve combustion stability. The higher temperatures resulting from higher pressure ratios will also be beneficial.

The use of kerosine might emphasise another trouble which has not been properly appreciated until recently. This concerns the possibility of danger from explosive mixture in the fuel tank. In the case of gasoline, although there is little danger of explosion there is a serious fire hazard when refuelling or in a crash. There is no great risk of explosion in flight because the vapour, which is formed in large volume in the tank above the surface of the petrol, is too rich to ignite easily and explode.

But in the case of kerosine, while it is safe to handle on the ground in temperate climates, it may not be so safe as gasoline in flight. If the aircraft tanks are well insulated with self-sealing material and the kerosine is at normal ground temperature in the tank, *i.e.*, +15°C., then there would be a possibility of explosive mixture in the tank space above the kerosine at about 12,000 metres (39,300 feet). Fig. 25 illustrates graphically, the explosive ranges of petrol and kerosine when related to temperature (in the tank) and altitude.

GENERAL REMARKS.

The following remarks are made at random and perhaps have no particular bearing on the main subject matter of this Paper. But they will exercise the gas turbine and also the aircraft manufacturers when some of the future aircraft begin their operating life.

The propeller turbine may present some difficulty in damping out the propeller noise because, unlike the piston engine, the turbine will cruise near to its maximum revolutions

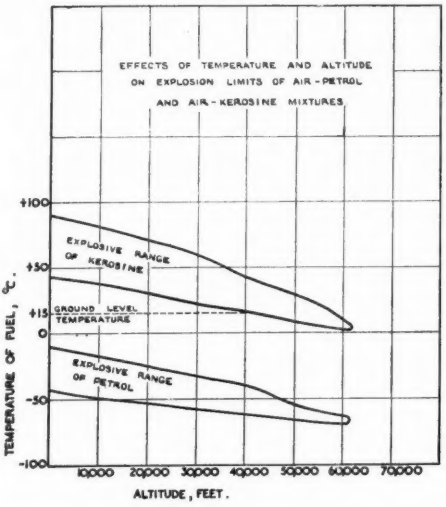

Fig. 25.
Risk of explosion in tanks containing petrol and kerosene.

and the propeller tip-speed may therefore be too high for passenger comfort, unless the propeller can be designed to give a tip-speed, at cruising conditions, of 650 feet per second (the accepted tip-speed for reasonable noise level) and still retain a good propulsive efficiency.

It is thought that the pitch change response, at constant speeding conditions, for propellers fitted to gas turbines should be more positive and "dead-beat" than that for piston engines. The reason for this is that the speed/power characteristics of a gas turbine are such that there is a very large increase in power over a narrow speed range as maximum speed is approached. Since propeller turbines will cruise somewhere in the region of 90 per cent. maximum revolutions, close control is considered essential in order to cope with large power differences.

The double reduction gearing necessary for propeller turbines must receive careful consideration in the design stages. Although they will not suffer the forcing (exciting) vibrations of the single reduction gears of the piston engine, trouble may be experienced because of high frequency vibration and resonance with turbine gears in the operating range. This type of gearing demands a high degree of accuracy in manufacture, accurate mounting, copious lubrication and the individual gears of the system must be well-proportioned and stiff.

In the case of very high-speed aircraft and those fitted with jet-turbines, the bi-fuel rocket and the propulsive duct or ram jet must not be forgotten. These will mainly concern military aircraft. They will be valuable and, I submit, will eventually be used as assisters and for supersonic flight.

I think it will be some years before passenger transport aircraft will travel at Mach numbers in excess of, say, 0.87 and, of course, it will be sometime before this figure is reached by this class of aircraft. In the case of piloted weapons for military purposes, however, the Mach number of 0.87 should be attained in a few years time and the next jump should reach beyond the sonic speed and go to a Mach number of 1.5 to 2, as the next stage.

ACKNOWLEDGMENTS.

In conclusion, my thanks go to the Ministry of Supply for permission to present this Paper.

I am also particularly grateful to Dr. Hawthorne and Mr. Moyes for their most valuable help and technical advice. And to S./Ldr. Golovine for the great assistance he has given me in useful criticism and in the original translation of the Paper into French

I also wish to thank the British engine manufacturers and their technical staffs for the data they have so freely given and allowed me to use in this Paper.

THE ROYAL AERONAUTICAL SOCIETY

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LOW DRAG AEROFOILS

by

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INTRODUCTION.

DURING the past few years a new series of low drag aerofoils has been developed which represents a radical departure from earlier practice. The changes envisaged are much greater than those which accompanied the general change-over from the biplane to the monoplane, and give rise to many problems whose solution requires considerable theoretical and experimental work. An important feature of the new sections is the precision in design and manufacture which is essential for their success. This has given renewed interest to the investigation of many of the detailed problems of air flow and calls for parallel improvements in manufacturing technique so as to achieve the high standard of surface finish required.

The purpose of this paper is to give a brief account of the theoretical basis of the design and application of the modified profiles as aircraft wing sections. It deals with the design of aerofoils for the subsonic range only, or, to be more precise, for flight at speeds below the critical Mach Number at which shock waves are first formed. The critical value usually lies in the range 0.6 to 0.8, depending on the wing shape and incidence, as will be described in more detail later.

NATURE OF WING DRAG.

Since the general adoption of the monoplane the main reductions of wing drag have resulted from decreases of wetted area or from flying at high altitude. Reductions in the wing drag coefficient have, until the advent of the new sections, produced only relatively small gains. Some decrease in drag coefficient has resulted for conventional wing sections from the removal of excrescences such as rivet heads and lapped joints, but it is now known that considerable further improvements can result from drastic reshaping of the wing section.

The basic factors affecting wing drag are first reviewed in order to show the directions in which reductions are possible. The total wing drag is usually sub-divided into induced drag and profile drag. The induced drag, which is associated with the production of lift, will not be considered in detail; the effects of the more important variables such as airspeed, span and planform on its value are well known. Attention will be concentrated on the consideration of the reduction of profile drag. This profile drag consists largely of skin friction which arises from the viscosity of the air and the considerable velocity gradients in the thin boundary layer adjacent to the wing surface. In addition to the skin friction drag there is also a small pressure or form drag.

INFLUENCE OF TRANSITION ON THE DRAG COEFFICIENT.

Near the nose of the wing the flow in the viscous boundary layer is usually steady or laminar but transition to turbulent flow occurs some distance back from the leading edge. The position at which this change takes place depends on a number of factors and its importance lies in the fact that the

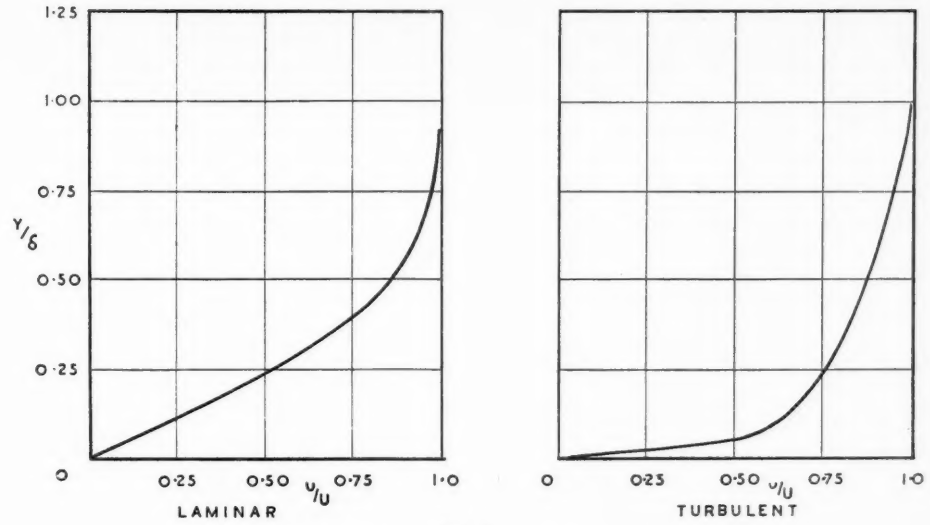


Fig. 1.
Boundary layer velocity profiles.

skin friction drag for laminar flow is much smaller than that for turbulent flow. Generally, laminar flow persists for a greater length at lower Reynolds' Numbers, but considerable changes can result from the presence of longitudinal pressure gradients in the flow. It is by control of such pressure gradients that reductions of profile drag are obtained.

For some time it was thought that early transition to turbulent flow was inevitable on full-scale aircraft and this gave rise to many suggestions for artificial boundary layer control. Such methods involved usually the removal of the air in the boundary layer by suction so that a new layer was formed. The new aerofoils postpone or eliminate the necessity for these devices.

The important differences between laminar and turbulent boundary layer flow are illustrated in the first two figures. Fig. 1 shows typical velocity distributions for both types of flow. It illustrates the much larger velocity gradients near the surface in the turbulent flow corresponding to the greater skin friction. Fig. 2 shows how the local intensity of skin friction varies along the upper surface of an aerofoil set at small incidence. The sudden increase in skin friction is associated with the transition from laminar to turbulent flow. The full line is typical of the older profiles with a transition point close to the nose, while the dotted line accords with a delay of transition to a point sixty per cent. of the chord from the nose. The reduction of skin friction drag due to

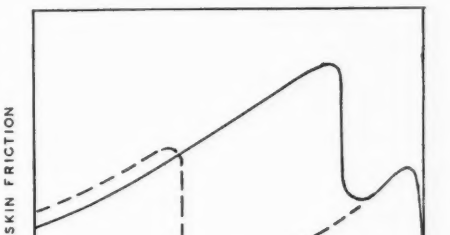


Fig. 2.
Variation of skin friction along chord.

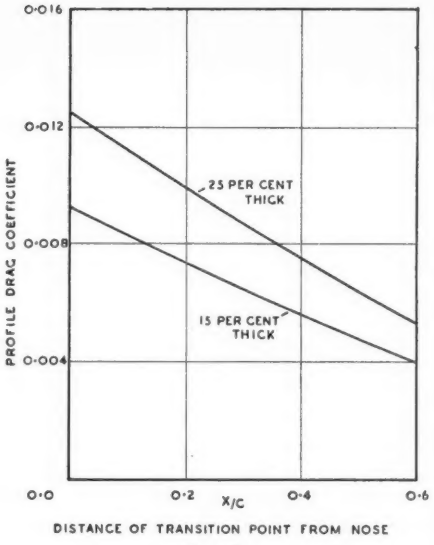


Fig. 3.

the rearward movement of the transition point is apparent.

Quantitative values for the profile drag coefficient are shown in Fig. 3. Here the drag coefficient for profiles 15 and 25 per cent. thick is plotted against the position of the transition points measured from the leading edge. The estimates shown are calculated by Squire and Young's method and apply to a Reynolds' Number of 10 millions. These curves do not vary greatly for different profiles so that they can be considered as representative. It will be seen on examination that the profile drag is roughly halved if transition can be delayed from about 10 per cent. of the chord up to 60 per cent. of the chord from the leading edge. Transition points even farther back than this have been obtained in experiment, so that a 50 per cent. reduction of profile drag can be considered as a practical possibility.

APPLICATION OF THE NEW SECTIONS

The realisation of far back transition points requires considerable modification of the profile shape and this often leads to some reduction of efficiency in other directions. In particular, some loss of maximum lift coefficient is likely to occur for both the plain and flapped wing. In addition, as has already been mentioned, a much higher standard of surface finish is required. Therefore, the most profitable applications of the modified sections will be to aircraft for which wing profile drag forms a considerable proportion of the total drag. This implies that the greatest gain is for fast, lightly-loaded aircraft flying at low altitude.

The importance of loading and speed on the magnitude of the possible gain is shown in Fig. 4. It indicates how the overall percentage reduction of the wing drag varies with the lift coefficient, assuming the profile drag to be reduced by 50 per cent. When the lift coefficient is small the wing drag is almost entirely profile drag so that the full 50 per cent. reduction is achieved. At higher lift coefficients the induced drag

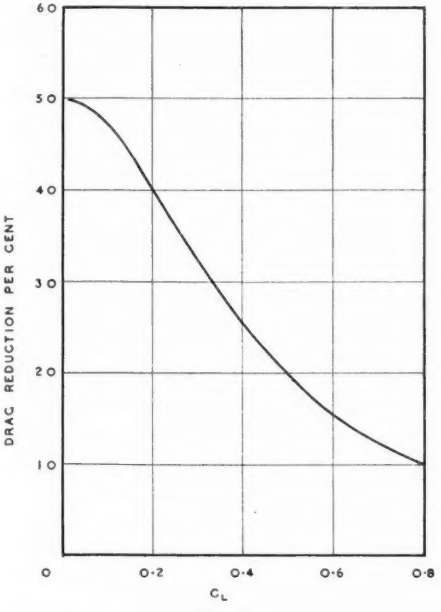


Fig. 4.
Reduction of total drag coefficient for a 50 per cent. reduction of profile drag.

becomes important so that reductions of profile drag have less effect. The curve shown applies to aspect ratio 7; values for other aspect ratios can be found by modifying the horizontal scale in the ratio $\sqrt{A/7}$.

Figure 5 shows for comparison the operating lift coefficients of aircraft under various conditions. The horizontal scale in this figure is the wing loading divided by the relative density, this can be readily interpreted when it is remembered that σ is about one half at 22,000 ft. altitude and one quarter at 40,000 ft.

These curves indicate that only a relatively limited advantage can accrue from the use of the new sections for heavily loaded aircraft flying at high altitude. But it must be remembered that such aircraft are usually designed for long range and therefore will fly for a considerable part of their operating life at loadings below the maximum. Also, the reduction of Reynolds' Number at high altitude makes it easier to maintain laminar flow; for instance, at 40,000 ft. altitude the Reynolds' Number is only roughly one-third of its value at low altitude for flight at an equal speed.

An upper limit to the speeds for which laminar flow aerofoils may be employed profitably arises from the production of shock waves at high Mach Numbers. The production of shock waves is usually associated with the attainment of velocities greater than the speed of sound near the wing surface. High-speed design aims mainly at delaying these effects as far as possible. To some extent this is incompatible with the main requirement for extensive laminar flow, although many of the profiles designed for the latter conditions are superior as regards high-speed performance to the older sections. Limitations of this nature arise at speeds above about 500 m.p.h. at low altitude or 450 m.p.h. at high altitude.

BOUNDARY LAYER FLOW

It might appear from the description already given, that minimum drag would occur if a laminar boundary layer extended

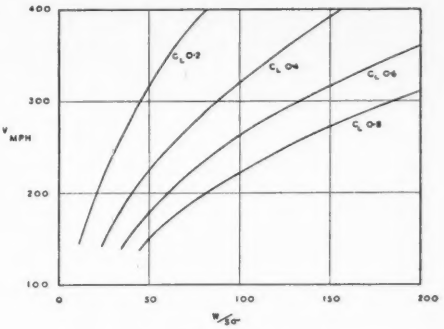


Fig. 5.
Variation of c_L with loading and speed.

over the whole wing surface. It can be shown, both theoretically and experimentally, that this is impossible.

At some point along the wing surface, depending on the profile shape and incidence, the phenomenon known as laminar separation occurs. Close to the wing surface the fluid whose kinetic energy has been partly dissipated by the viscous tractions in the boundary layer, is brought to rest by a retarding pressure gradient. At small scale, the fluid which previously formed the boundary layer leaves the surface and a wide eddying wake results. At larger Reynolds' Numbers, if the laminar layer extends so far, laminar separation is followed by transition to turbulence and the formation of a turbulent boundary layer. The pressure gradients over the rear of the aerofoil at small incidence are usually insufficient to bring about separation of a turbulent boundary layer so that only a thin wake is formed behind the aerofoil.

The laminar separation point thus marks the rearward limit of steady boundary layer flow, but a laminar layer will extend so far only under especially favourable conditions. A more forward transition point usually occurs in practice.

CONDITIONS FAVOURABLE TO DELAYED TRANSITION

It has been found experimentally that transition to turbulence is delayed by an

accelerating pressure gradient acting along the boundary layer, in other words, if the velocity just outside the boundary layer increases continuously from the nose.

A striking example of this effect is shown by an experiment of Dryden's in a moderately turbulent wind tunnel. Tests on a flat plate in the bare tunnel gave a transition Reynolds' Number of 1.8 million (reckoned on the distance from the leading edge). Because of the thickening of the boundary layer on the tunnel walls, the velocity down the centre of the tunnel increased by 2½ per cent. over the length of the plate. Thus a small accelerating pressure gradient was present in the initial experiment. When this pressure gradient was eliminated in further tests the transition Reynolds' Number dropped considerably, falling to 1.1 million.

In a wind tunnel the fluctuating pressure gradients in the airstream are responsible for the eventual transition from steady to turbulent flow, the transition point receding from the nose as these disturbances are reduced. In flight, the air is effectively non-turbulent but the excellent surfaces employed in wind tunnel work are not readily attainable. Local decelerating pressure gradients may arise well forward on the wing due to surface irregularities and, if these lead to local separation, transition to turbulence will result. The effects of initial turbulence in the air flow and surface irregularities are similar in so far that transition arising from both causes is delayed by an accelerating pressure gradient.

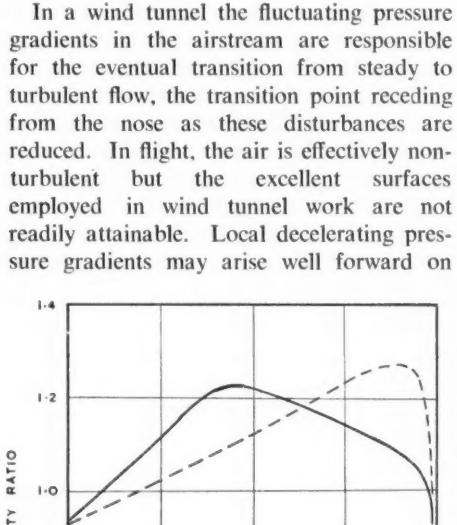


Fig. 6.
Comparison of velocity distributions for laminar flow and conventional sections.

The general requirement for the maintenance of laminar flow is therefore as follows. For a considerable distance back from the leading edge the pressure should decrease continuously. A typical velocity curve for a symmetrical aerofoil is shown by the full line in Fig. 6 and is compared with the velocity distribution shown dotted for a conventional profile with its maximum thickness located at 30 per cent. of the chord from the nose. The accelerating pressure gradient should continue only as far as the laminar layer can be maintained. It is useless to continue an accelerating pressure gradient after transition has occurred since this will only lead to a more rapid drop in velocity near the trailing edge. The turbulent boundary layer will then be thicker and may even break away from the surface, thus resulting in higher drag. For instance, in positions where transition inevitably occurs well forward on the wing, such as behind airscrews, there is no aerodynamic advantage in employing laminar flow sections.

The problem of designing an aerofoil for specified conditions is more involved than that of merely providing the required type of velocity distribution at a single incidence. On a long flight considerable variations of lift coefficient can occur due to fuel consumption and it is desirable that the favourable properties should exist over a sufficient range of incidence to cover this variation. Further, the performance at high incidence should not be too greatly impaired.

THEORETICAL METHODS OF DESIGN

Before considering in detail sectional shapes which go far to fulfil these requirements it will be useful to discuss briefly the methods of calculation employed. A large part of the theoretical work relates to the

calculation of the inviscid flow past long cylinders of aerofoil cross-section. The actual flow outside the thin boundary layer is assumed to be identical with that calculated. Thus we neglect the small outward displacement of the streamlines which arises from the retardation of the fluid in the boundary layer and which gives rise to the small pressure drag present in the actual flow but absent in the theoretical flow. The assumption that the effects of viscosity are negligible outside the boundary layer is justified by a considerable amount of experimental evidence. The major differences between experiment and theory occur near the trailing edge where the boundary layer thickness is greatest. For this reason the theoretical velocity curves in the further figures have been terminated a short distance from the trailing edge; they have little practical significance close to the tail.

A further assumption is implied in the application to a wing of limited span of results which are calculated for a long cylinder of constant cross-section. The assumption in this case is a familiar one forming the basis of the Lanchester-Prandtl aerofoil theory. It is that the flow over the three-dimensional wing differs from the two-dimensional flow only by the induced velocities arising from the trailing vortex system. This yields satisfactory results except towards the wing tips where the flow takes on a marked three-dimensional character.

In recent years considerable advances have been made in the methods of calculating the two-dimensional inviscid flow past aerofoil sections of a wide range of shapes. Much of this work done at the Royal Aircraft Establishment and the National Physical Laboratory has been specifically related to corresponding experimental work. Professor Goldstein, working at the National Physical Laboratory, has further developed Theordorsen's method by which the flow past any arbitrary profile is calculated approximately.

A team headed by Dr. Piercy, and of which the author has been a member, has followed a rather different line of development. Starting from the simple Piercy aerofoils, first published in 1937, the method of conformal transformation then used has been greatly developed so that the effects of any practical modification of profile shape now comes within its scope.

SYMMETRICAL PROFILES

The major change required from the older wing sections consists of the rearward displacement of the position of the maximum thickness of the profile. This will be considered in detail for symmetrical aerofoils set at zero incidence.

Figure 7 shows the calculated velocity distribution over three symmetrical sections, all 15 per cent. thick but differing in the position of the maximum thickness. The maximum ordinate is located at 35 per cent. of the chord from the leading edge for the aerofoil A, 45 per cent. for B, and 55 per cent. for C. The first profile has its maximum thickness some 5 per cent. of the chord farther back than most of the older sections, although its velocity distribution is not improved by this change. A more drastic modification is required, as is shown by the two other curves. The aerofoil B shows

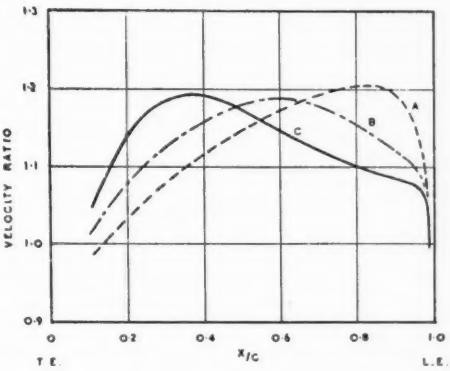


Fig. 7.
Variation of velocity distribution with position of maximum ordinate.

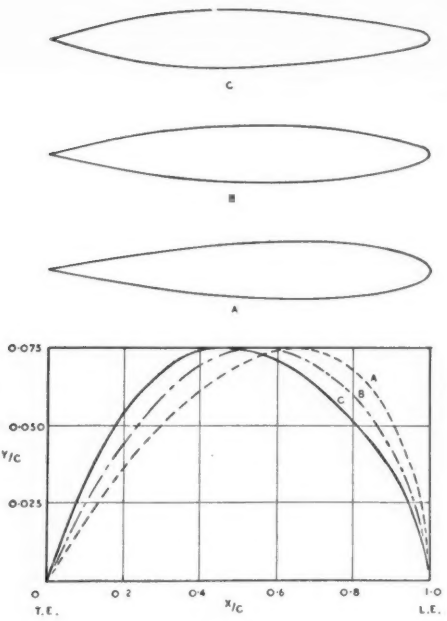


Fig. 8.

some change in the required direction, but only C with its maximum thickness 55 per cent. of the chord from the nose has an accelerating pressure gradient over a really extensive part of the profile. Thus the maximum thickness of the section must recede to at least 45 per cent. of the chord from the leading edge and preferably a further 10 per cent. of the chord back.

Figure 8 compares the cross-sectional shapes of these symmetrical aerofoils, the lower curves showing the half sections on a magnified scale. Two factors associated with the rearward displacement of the maximum thickness are immediately apparent. The first of these is a considerable sharpening of the nose and the second an increase in tail angle.

The sharpening of the nose is essential for the realisation of the required velocity curve; any attempt to increase the radius of curvature appreciably leads to a reversal of the pressure gradient near the nose with the consequent risk of early transition. Fig. 9 illustrates this point; it shows the velocity distributions at three angles of incidence for two aerofoils which are identical except in the immediate vicinity of the nose. The lower set of curves relates to an aerofoil for which the radius of curvature at the nose is some 20 per cent. less than for the upper curves. The curves for zero lift indicate a much smaller pressure gradient near the nose for the upper aerofoil and any further increase of radius would lead rapidly to a reversal of pressure gradient. The other curves for lift coefficients of 0.1 and 0.2 show this more clearly and also illustrate another important factor. The aerofoil with the sharper nose can be set at a larger angle of incidence without producing a reversal of pressure gradient at the nose.

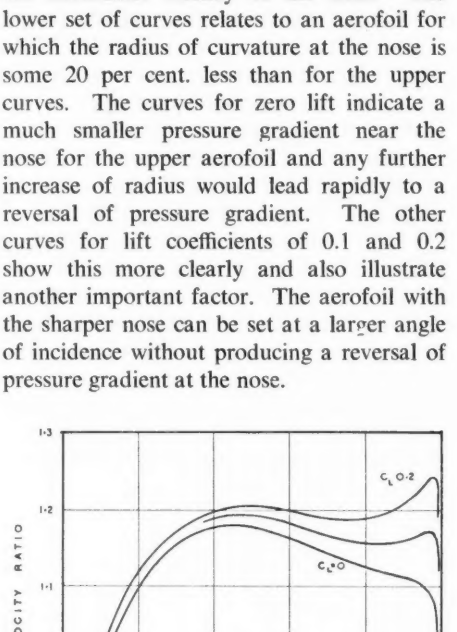


Fig. 9.
Influence of nose curvature on velocity distribution

The effect of reducing the nose radius is therefore to extend the range of incidence over which it is possible to maintain laminar flow over the whole of the forward part of the profile. Similar effects have been found for cambered aerofoils, the so-called favourable range of incidence then has its centre at a lift coefficient depending on the camber instead of, as in the present example, at zero lift. At first it might appear surprising that sharpening the nose should prevent a velocity peak occuring in this region. The effect is limited, however, to small incidence and the expected increase in velocity near the nose with sharpness occurs when stalling lift coefficients are approached. Unfortunately, experiment suggests that sharpening the nose leads to a reduction of the maximum lift coefficient, so that while a small radius is highly desirable its reduction cannot be carried to extremes.

Figure 10 shows the effect of thickening up an aerofoil on either side of the maximum ordinate. The thickened aerofoil has a smaller accelerating pressure gradient and the figure indicates the changes in shape required to alter its value.

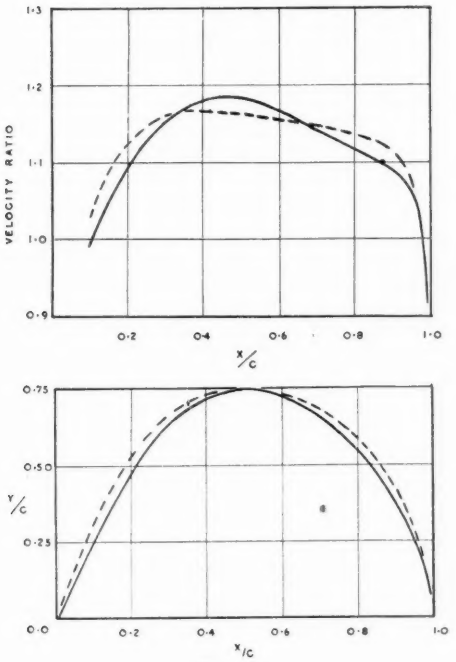


Fig. 10.

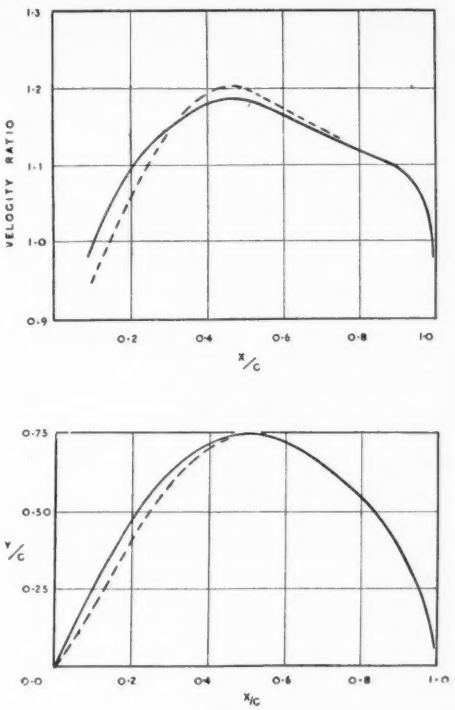


Fig. 11.

Figure 11 shows that the large increase of tail angle consequent upon moving the maximum thickness back can be partly avoided if a small reversal of curvature near the trailing edge is acceptable. The figure compares one of the sections previously shown in Fig. 9 with another which is practically identical over the forward part of the profile but has a reduced thickness towards the tail. The change increases the maximum velocity over the section and correspondingly adds to the accelerating pressure gradient. This type of section may be preferable for some purposes. For instance, the flow over ailerons is likely to be improved as it is difficult to ensure satisfactory operation with large tail angles.

CAMBERED PROFILES

Symmetrical profiles are only suitable for use at small lift coefficients, for any considerable increase of incidence leads rapidly to a velocity peak near the nose. This effect arises partly from a concentration of the lift over the forward part of the profile, evidenced by an increase in the velocity on the upper surface in this region and a reduction on the lower surface. With suitable camber such concentration of lift can be avoided and accelerating pressure gradients preserved on both surfaces.

The design of cambered profiles can be approached from two points of view; either we can endeavour to maintain equal velocity gradients on the two surfaces, or we can allow some concentration of the lift towards the nose and so reduce the velocity gradient on the upper surface while increasing it on the lower surface. The second line of attack

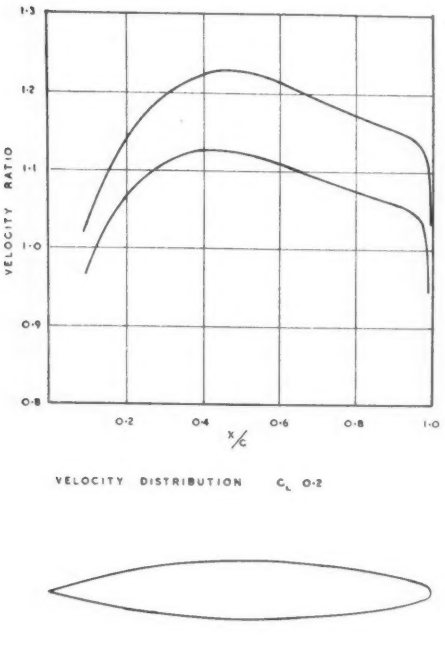


Fig. 12.

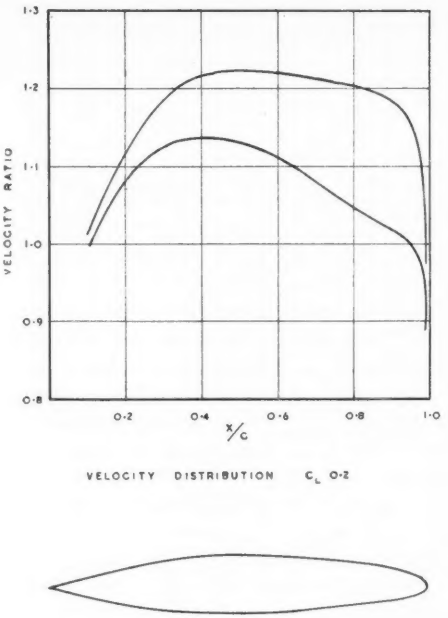
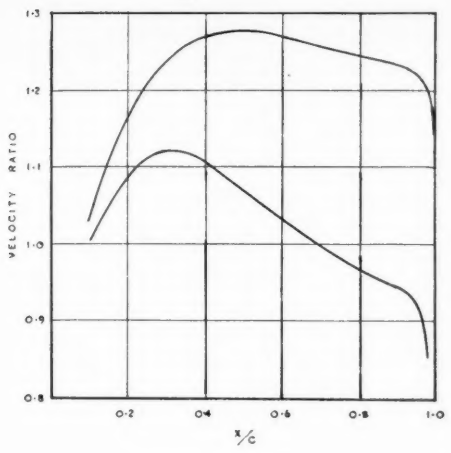


Fig. 13.

will lead to smaller values of the moment coefficient. But it is clearly impracticable when the symmetrical aerofoil from which the section is developed has only a small accelerating velocity gradient.

Illustrating the first line of approach, Fig. 12 shows a one per cent. cambered aerofoil with a circular arc camber line. At a lift coefficient of 0.2 it gives approximately equal velocity gradients on the two surfaces over the forward part of the aerofoil. The section is 15 per cent. thick with the maximum ordinate located at the centre of the chord.

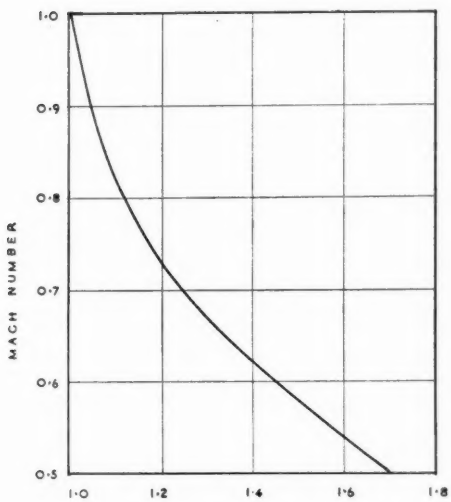
An alternative one per cent. cambered aerofoil in which the crest of the camber line is moved to 30 per cent. of the chord from the nose, is shown in Fig. 13. It carries the second line of development to the limit. The moment coefficient at zero lift, which is a convenient measure of the centre of pressure travel, is reduced by the change to one-third. Compared with the previous aerofoil,



VELOCITY DISTRIBUTION C_L 0·4

2 PER CENT CAMBERED SECTION

Fig. 14.



INCOMPRESSIBLE FLOW VELOCITY RATIO

Fig. 15.

it may not be possible to maintain laminar flow over such a great length of the upper surface because the accelerating pressure gradient is reduced. An increase in the extent of the laminar layer on the lower surface is to be expected and should compensate for the loss on the upper surface.

The third cambered section shown in Fig. 14 has 2 per cent. camber and a less drastic reduction of moment coefficient than for the second 1 per cent. cambered section. The maximum thickness of 15 per cent. is in this case 55 per cent. of the chord from the leading edge. The curves shown are for a lift coefficient of 0.4 and are generally similar to those for the previous aerofoil with a lift coefficient of one half this value.

COMPRESSIBILITY LIMITATIONS

The high speed limitations imposed on aerofoil design will now be considered in more detail. When the forward speed becomes so high that shock waves are formed, large increases of drag occur. Under these conditions it becomes more important to delay the onset of shock waves than to preserve laminar flow. The velocity curves shown all relate to incompressible flow and progress towards the more difficult problem of the calculation of compressible flow usually proceeds by determining approximate corrections to be applied to the incompressible flow solution. The general effects of compressibility below the shock stalling speed are to increase the magnitude of the velocity ratio at a given incidence, without greatly changing the general shape of the velocity curve. The curves already shown, which apply accurately at lower speeds, will therefore be qualitatively correct for this part of the high-speed range.

A simple method of representing one aspect of compressibility effects is shown in Fig. 15. The maximum velocity ratio, for incompressible flow past the section is plotted against the Mach Number at which the speed of sound is first reached locally on the wing surface. The curve takes into

account both the increase in the velocity ratio due to compressibility and also the local reduction of the speed of sound.

Design to delay compressibility effect aims, therefore, at decreasing the maximum velocity ratio for the section. Reduced values of the velocity ratio occur when the pressure gradients are small, so that the requirements differ from those for extensive laminar flow. The 15 per cent. thick symmetrical sections which have been described all have maximum velocity ratios lying in the range 1.18 to 1.20, whereas for the elliptic cylinder the value is 1.15 and a reduction to about 1.14 is theoretically possible. It is not suggested that the profiles producing these minimum values are of more than academic interest, but they point the way to the design of practical sections with a maximum velocity ratio little greater than 1.15. Increasing the critical Mach Number in this manner by careful design is usually more suitable from a structural viewpoint than the only alternative of reducing aerofoil thickness.

Increases in the critical Mach Numbers for cambered aerofoils are also possible by reducing the pressure gradients. Higher critical Mach Numbers than 0.7 for the one per cent. sections will then be possible. The value 0.7 corresponds for flight at 40,000 ft. altitude to a speed of 470 m.p.h. and a wing loading up to 70 lb. per sq. ft. Reduction of loading for these sections will not greatly increase the critical speed as the maximum velocity ratio is not very sensitive to slight changes of angle at small incidences.

In conclusion it may be useful to refer again to the possibilities of boundary layer control as a means of extending laminar flow at very high Reynolds' Numbers. For very large aircraft it may be practically impossible to maintain a sufficiently extensive laminar layer even with the modified wing sections. If, however, the boundary layer is removed by suction near the limit of laminar flow a new boundary layer will be formed downstream behind the slot. The Reynolds' Number of such a layer would then be calculated correctly in terms of the distance from the slot and not from the leading edge. The reduction of effective Reynolds' Number so obtained should make it easier to maintain laminar flow beyond the slot in the presence of an accelerating pressure gradient.

ASPECTS OF THE DETERMINATION OF THE STRENGTH OF MATERIALS

The following is a summary, by Dr. E. W. J. Mardles, of some of the main points which were discussed at two meetings held by the British Rheologists' Club in June, 1945, and February, 1946. Dr. Mardles is Secretary of the Club.

SOME aspects of the determination of the strength of materials formed the subject of two meetings held jointly with the British Rheologists' Club (President, Prof. E. N. da C. Andrade) and the Royal Aircraft Establishment in the Assembly Hall, Farnborough, on the 16th June, 1945, and with the Royal Aeronautical Society in the library at 4, Hamilton Place, on the 19th February, 1946, under the chairmanship of Sir Ben Lockspeiser.

At the Farnborough meeting three papers were read, namely, "The investigation of failures in wood by microscopical examination" by M. C. Pryor and A. Rayne; "The effect of duration of loading on the strength of brittle materials" by C. Gurney; and "Application of statistical methods to mechanical test results" by B. Chalmers and E. R. W. Jones. At the London meeting Prof. N. F. Mott of Bristol University spoke on the "Griffith theory of cracks in solids and recent developments of this theory, with application to brittle fracture in glass and in metals."

Dr. M. C. Pryor, in a paper entitled "The investigation of failures in wood by microscopical examination," spoke on the molecular and micro structure of timbers used in aircraft construction, and of the micro methods employed after an accident to establish the cause of mechanical failure.

The movement of micelle aggregates during deformation was accompanied by rupture with the formation of fresh surfaces between the lignin and cellulose portions in the strained and damaged areas, and these fresh surfaces could be selectively stained. Failures by compression or tension were easily distinguishable microscopically, as originally shown by W. Robinson (*Phil. Trans.*, B, 210, 49; 1920). Under compression, the tracheids in the zone of failure were considerably buckled. The zones of failure, which usually were well defined, ran in planes at an angle of about 45° to the compression stress. Under tension, failure developed along slip planes in the substance of the cell walls, and these local shear failures occurred in isolated points where the tube wall was weak, so that the failures were scattered up and down the members over quite a wide distance, perhaps for several inches.

In his paper Mr. Gurney said that some brittle materials, particularly mineral glasses, were known to be subject to static fatigue, the breaking load decreasing with duration of loading; for example, experiments on an annealed soda-lime glass showed that the strength decreased in the ratio 3 : 1 when the duration of loading was increased from 10^{-2} to 10^{4} sec.

It was generally accepted that the weakness of brittle materials was due to cracks, as suggested by Griffith. A likely explanation of the time dependence of strength might be the gradual spread of cracks; Griffith, after considering the energy conditions necessary for the spread of cracks, concluded that crack-spreading could not occur until a critical load was reached; at this load the crack spread catastrophically, and failure was immediate. In these circumstances, the

only previous explanation of failure after prolonged loading which was consistent with general principles was damage to the surface material by atmospheric attack. If glass was rapidly broken, there might be no time for atmospheric attack of the growing surfaces of the crack, so that a relatively high strength corresponding to the uncontaminated surfaces was obtained.

A number of processes by which cracks might spread, consistent with the laws of thermodynamics, were described, and a new estimate of the strength of flawless materials was given. Stress reduced the height of atomic energy barriers, and immediate failure of a uniformly stressed material would occur when the average thermal motion was just sufficient to overcome the average energy barrier. This condition was approximately that at which the latent heat of isothermal evaporation was zero.

At lower stresses, failure was still possible, but it took time. The stress-free vapour pressure of strong solids was extremely small, but it had been shown that the logarithm of the vapour pressure was approximately proportional to the square of the stress, so that the vapour pressure became appreciable at high stresses such as could occur at the end of cracks.

The rate at which cracks spread by evaporation of the material had been estimated and found to be insufficient to account for the whole of the time effect with mineral glasses, tested under atmospheric conditions. In a material the atomic arrangement of which was in thermal equilibrium, increase in crack-length by splitting without atomic migrations could only occur at loads in excess of that given by the Griffith criterion. It had hitherto been thought that failure was immediate at the Griffith load, but it had been shown that, for materials having atomic constitution, the Griffith load was the lowest at which the crack might start to spread by splitting, the rate of spread being a function of the stress difference between that at the end of the crack and the maximum the material could withstand.

For materials not in thermal equilibrium, the application of stress by reducing the height of energy barriers enabled the approach to equilibrium to continue; and if this involved heterogeneous volume changes, internal stresses were set up which caused cracks to spread. Atmospheric attack, involving weakening of the surface material and possible solution of material at the ends of cracks—solubility increased very rapidly with stress—was likely to be an important factor in causing fracture under prolonged loading, when tests were made under ordinary atmospheric conditions.

In the discussion which followed, Dr. E. Orowan spoke on the mechanism of crack spreading and emphasised the importance of atmospheric attack. Dr. Maunder Foster mentioned the high stresses produced during grinding and their effect on reduction of molecular weight.

During the afternoon session, Dr. B. Chalmers, in a paper with Mr. E. R. W. Jones, on the "Application of Statistical Methods to Mechanical Test Results," gave a brief account of reasons for adopting a statistical approach to the interpretation of mechanical test results; and examples were given of significance testing, assessment of correlation and curve-fitting in relation to experimental results.

Strength tests were in general destructive, so that it was impossible to apply them to the component to be used. When they were non-destructive, the measurement was not of the property under consideration but of another property in some way related to it. In either case one required a basis for inferring mechanical properties of the component from the known properties of the specimen tested. It was pointed out that the three reasons for doing mechanical tests were: (*a*) to provide design data; (*b*) to determine whether two or more batches of

material were significantly the same in the mechanical property examined; and (*c*) to provide the basis for establishing the rheological laws for the material.

It was emphasised that, in all statements of the numerical value of a quantity, it was desirable to quote both the mean and an estimate of the scatter, based on the standard deviation.

The importance of using impersonal methods for fitting curves was demonstrated by an example in which significantly different results were obtained by various observers interpolating on the same experimental points. In conclusion, a method of presenting results of fatigue tests was shown giving the stresses below which only a stated percentage of specimens would be expected to fail. The view was expressed that this was the logical method of expressing such information in cases where the data were to be used quantitatively for design purposes.

In a continuation of the discussion at the meeting in the Society's library in February, 1946, Prof. Mott, in introducing the subject of strength of materials, pointed out that normal materials seldom surpassed in strength a small percentage of the theoretical and the question arose "How did solids break?" Hard brittle solids like glass broke by fracture, while steel broke after yielding and the phenomenon of the plasticity of steel like that of superconductivity appeared to be without adequate explanation.

A. A. Griffith advanced the theory that brittle solids broke by cleavage at the ends of incipient cracks at the surface, presumably caused during ageing by surface adsorption stresses.

Prof. Mott described experiments in which he had determined the tensile strength of a large number of steel specimens of the same material. He had found that these broke in the tensile machine at the neck (50 per cent. reduction in area), each in practically the same way, a behaviour distinctly different from that of glass when a big scatter in results was obtained.

The reproducibility of results for the fracture of steel under stress was a most striking behaviour and one difficult to explain. When the conditions of fracture of steel were altered to an extremely rapid application of stress, as in the fragmentation of shell cases, then an interesting behaviour was found. In the fragmentation of shell cases the steel in rapid expansion disintegrated after a plastic expansion of about 30 to 40 per cent., the fragments acquiring their velocity before fracture and the number of fragments being a function of the rate of expansion.

Prof. Andrade, speaking on the subject of the self-healing of cracks, mentioned experiments he had made with freshly broken glass rods immersed in sodium vapour. The sodium vapour etched the glass at the crack formations producing fine visible lines.

Dr. Orowan of the Cavendish laboratory described experiments on the removal of surface cracks: when glass rods had been washed in hydrofluoric acid their tensile strength increased enormously; similarly, when the strength of crystals of salt were determined in water, which dissolved the cracked surfaces, much higher values were obtained than in air. Similarly the strength of glass fibres was higher when kept in vacuo or protected by oil; Griffith found the strength of newly drawn fibres to be twenty times or so the usual strength, but these high values fell rapidly when the fibres were handled or kept.

Dr. Treloar pointed out that there was rather a close analogy between the process of fragmentation of an exploding shell case considered by Prof. Mott and the mechanism of bursting of an inflated rubber sheet. It was discovered by Naunton and Flint that a circular sheet of rubber, clamped round its circumference and inflated, burst so as to form a series of radial tears or cracks radiating outwards from the centre. Examining the phenomenon in more detail Dr. Treloar found that the number of radial cracks obtained with different rubbers

increased as their tensile strength and effective modulus of elasticity increased.

In explanation of these effects it was suggested that the number of cracks would depend on the relative rates of propagation of the elastic relaxation in the neighbourhood of a crack and the rate of propagation of a crack; thus, there would appear to be a close similarity between the bursting of a rubber sheet and the bursting of a steel shell discussed in more detail by Prof. Mott.

Dr. Haward referred to the splintering of glass. If similar glass plates were broken by the method of a gradually increasing blow, there was a wide variation in the energy required to cause fracture from one piece to another, and what was important, in those cases where the energy was greatest there would generally be the greatest number of radial cracks. A somewhat similar observation with regard to clay plates had been made by Suzuki (Proc. Phys. Math. Soc., Japan, 3-4, 168, 1921-2). This could not be due to the variation of the elastic modulus at the time of breaking, but was more likely to be connected with the strain energy in the glass at the time of fracture. In connection with Prof. Mott's interesting experiments on bomb fracture, it would appear that if the shell burst as a result of the momentum acquired by the casing before fracture, the velocity it acquired would influence the result, in that appreciable stresses would be necessary in order to change the direction of motion of different radial elements of particular pieces of the casing so that they might continue to move as a coherent whole, instead of each sector moving outwards along a radius.

In subsequent discussion several speakers referred to the time factor, pointing out that with increase of time there would be greater self-healing of cracks as would occur with pitch, bitumen and other plastic substances.

Sir Ben Lockspeiser, in concluding the discussion, indicated a new angle of approach to strength problems. In working with A. A. Griffith they had considered that classical thermodynamics were in general unsuccessful in predicting the freedom possessed by systems which were partly or wholly solid.

With pure iron, overstrain caused changes in all the physical properties measured, indicating the occurrence of some kind of phase change. Gibbs' phase rule extended to solids as polyphase materials indicated that five additional degrees of freedom were conferred in virtue of the capacity of the solid to withstand shear stresses.

CORRESPONDENCE

TO THE EDITOR, *7th December*, 1946.

NITROBENZENE DANGERS

In my Wilbur Wright lecture in May, 1946, I mentioned the use of a china clay coating on wind tunnel models, subsequently sprayed with nitrobenzene, as an effective means of making visible the demarkation between the laminar and turbulent parts of the boundary layer. It has since been pointed out by the Chemistry Division of the R.A.E. that nitrobenzene is highly toxic. I quote the relevant paragraph from their report:—

> "Nitrobenzene is one of the most toxic substances known. Sir Thomas Legge, Medical Adviser to the Trades Union Congress and late Senior Medical Inspector of Factories, gives the 'maximum allowable for prolonged exposure' to nitrobenzene vapour as 1/500th part per 10,000 parts of air—equivalent to 1 part in 5 millions of air. The same toxicity is given by Fieldner, Katz and Kinney. This makes nitrobenzene the most toxic substance known, and 5 times as toxic as phosgene and 500 times as toxic as carbon monoxide. Henderson and Haggard give the toxicity as 1 to 5 parts per million, the same as phosgene. We do not think that this American figure would be acceptable to our Home Office."

It is evidently desirable to avoid the use of nitrobenzene, and this letter is written to give the facts a wider circulation. Fortunately other substances can be used, it being merely necessary to choose one which evaporates slowly and whose refractive index is near that of the china clay crystals. Substances which have been suggested, in order of increasing slowness of evaporation, are ethyl benzoate, methyl salicylate, ethyl salicylate, safrole, chloronaphthalene, Iso safrole, and eugenol, and some of these are doubtless quite safe.

One can also use the technique devised by W. E. Gray at R.A.E., in which the model, preferably black, is wiped with a rag carrying an oil such as kerosene. The differential evaporation of the oil shows the turbulent region quite well under suitable illumination, although the contrast is nothing like so striking as it is with the china clay method.

Yours faithfully,

ERNEST F. RELF, C.B.E., F.R.S., Fellow.

BOOK REVIEW

Human Factors in Air Transport Design.

Ross A. McFarland. McGraw-Hill Company, Inc., New York and London, 1946. 670 pp. $6.50.

The successful development of any machine cannot leave out the ultimate object of that machine, its use by man. The human factor plays an increasing part with the increasing use of the machine. In the early days of the motor car, for example, the human factor was neglected. Now, and rightly so, the human factor has become dominant, from the provision of fool-proof devices, ease of driving, and reliability, to the comfort of passengers.

These factors have long been recognised in the development of aircraft, but the effort to solve the problems involved has been somewhat spasmodic in the past and certainly not

co-ordinated. A comprehensive study of how the human factor affects the design of aircraft, particularly transport aircraft, has long been overdue.

Dr. McFarland has now made that study, and it can be said at once that this is a book which should be read with great care and considerable profit by all those concerned with the design, development and operation of air transport machines.

The chapter headings themselves are almost a sufficient indication of the wide range of the volume under review. They are: The Trends and Characteristics of Air Transportation; High Altitude Operations and Pressurised Cabins; The Control of Ventilation, Temperature and Humidity; The Control of Insects and Airborne Diseases; Carbon Monoxide and other Noxious Gases; The Control of Noise in the Cabins of Aircraft; The Control of Vibration in Air Transport Planes; Acceleration, Motion and Flight Performance; The Cockpit and Control Cabin of Air Transports; Passenger Accommodation on Air Transport Planes; and The Prevention of Aircraft Accidents.

Each chapter ends with a summary and recommendations of what should be done to increase the efficiency of the aircraft from the point of view of the pilot, passenger and designer. And each chapter contains an excellent selected biography.

It is not possible to pay adequate justice to this remarkable volume in the space which can be given to a review. There is this to be said about it, that it contains sufficient material to pay designers over and over again to buy the volume and study it, with all the implications behind it.

It was stated earlier that each chapter ends with a summary of the facts and recommendations. The final chapter summarises the conclusions in a broad way and ends with the following:—

"The application of scientific thinking in regard to human factors can be more successfully achieved by introducing some of the relevant aspects of the biological sciences into the aeronautical engineer's course of study. In addition, the major manufacturers might establish aero-medical units or teams of scientists with training in human problems as an integral part of the engineering and design departments.

"A clearer definition of the human problems could be obtained if comprehensive time and motion studies of the tasks of air crews were carried out. Similarly, more definitive studies of the activities, responses, and needs of the passengers are required to isolate the pertinent variables influencing the human organism in flight.

"The time lag between research, especially in biological fields, and its application to aviation might be reduced greatly by establishing a central co-ordinating agency for the exchange of information, not only between the operators and manufacturers but also between each of these groups and government and university research laboratories.

"Finally, the basic research for fundamental human applications in design can probably be best obtained by liberally supporting, with both funds and trained personnel, the existing and proposed research units in federal agencies and academic institutions. Such units can improve their effectiveness by international collaboration with similar groups in other countries since scientific progress is no more limited by national boundaries than the air transport itself."

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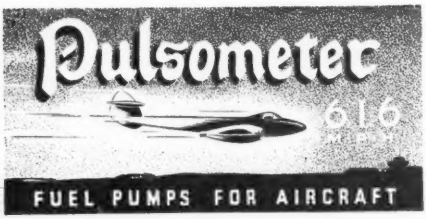

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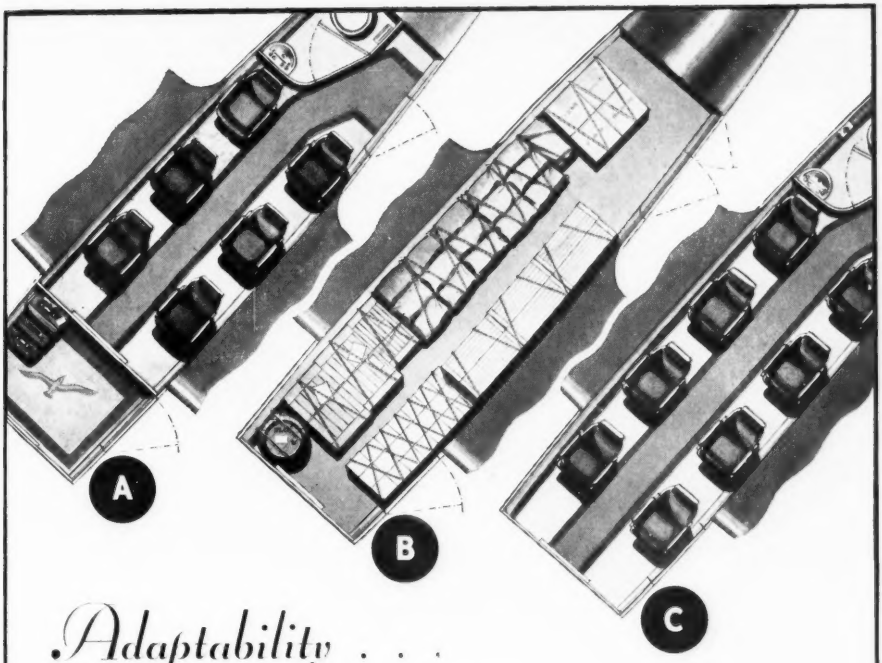

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